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SOME PHYSIOLOGICAL ASPECTS OF HYPNOTISM.[1]

MENTAL phenomena may be viewed from two entirely different standpoints. With one of these aspects, that furnished by the introspective method, a physiologist has no business to meddle, but the other, in which the Brain is considered as the organ of mind, forms a most important part of the Physiology of the Nervous System.

Mind is undoubtedly correlated with the existence of special living structures; the processes which constitute, in physiological parlance, the functional attributes of nervous tissues are transmuted in the crucible of consciousness into the various moods of sensation, thought and volition. The *rationale* of such transformation is as inexplicable as that of the origin of matter or the commencement of life.

From the physiologist's point of view, the transmutation is one in which material changes capable of investigation by physical methods are suddenly associated with the presence of immaterial phenomena, the characters of which his methods are impotent to decipher. He recognises that this condition which has suddenly burst into blossom is, from another point of view, existence itself, the conscious something provoked from senseless nothing, but, in spite of this recognition, he knows that the presence of the mental state

[1] A Lecture delivered at Magdalen College, Oxford, March, 1897.

is to be regarded by him merely as a sign indicative of the full development of those underlying physiological processes which are the object of his investigation.

The phenomena of Hypnotism may be viewed from such a standpoint, the mental conditions being used as signs of the development of physiological states. The subject is one of great interest to both physiologists and psychologists, since the curious phenomena of the hypnotic state appear to be focussed around two phases of mental mood which are themselves of fundamental importance, "attention" and "suggestion"—the first a predominant factor in determining intellectual growth, the second a determinant of the manifestation of volition. The profound modification which the cerebral activities undergo through the production of the hypnotic state brings these two aspects of mental conditions into special prominence, and thus the phenomena of hypnotism may not improbably provide a clue towards the better realisation of the presumed physiological processes which underlie these and other mental traits.

It must be confessed that our knowledge of such processes is most limited and vague, and that, as regards their fundamental nature, we are at present only able to make hypothetical inferences; still the inference, even if hypothetical, is strengthened by the circumstance that it is the transference to the highest portion of the nervous system of details more or less well grounded in connection with lower portions of this system, and it gains in consequence of its logical character, more than it loses by the hypothetical element involved in the transference.

A physiological aspect of Hypnotism must therefore ıvolve the special group of phenomena as part of nervous henomena in general, and the object of the present article to set forth the extent to which modern views as to nerve tructure and nerve activity can be utilised for throwing ıght upon the special physiology now under consideration. At the outset it is necessary to clear the ground by stating, in a very general way, what physiological conditions are held by the writer to denote the presence or absence of volitional power; such a statement is convenient for the

handling of the subject, but it must not be taken as in any sense a definition of volition.

Sensation or muscular activity in some form are the only certain indications to us of central nervous activity. When a specific form of such muscular activity is always obtained in response to a perfectly definite mode of sensory stimulation, then voluntary power is held to be in abeyance; the absence of the power of modifying the muscular response to specific sensory stimulation thus becomes the important test for determining whether the animal experimented upon has or has not volitional power. However closely an animal's actions resemble those in which volition is an undoubted concomitant, they must, in the absence of such possible modification, be termed automatic. Volitional paralysis thus involves inability to modify the response to definite sensory stimulus, hence such paralysis may result in the display of automatic activity, and this plays a prominent part in the production of hypnotic phenomena.

With this prelude we may proceed to consider quite briefly, some of the chief characters of the phenomena and the means by which they may be produced.

If a facetted piece of glass, a crystal or some bright object is held a few feet from a person's eyes and slightly above their level, and visual attention concentrated upon the object, then, after a variable time, a change may take place in the gazer. The pupils instead of remaining constricted, as they do involuntarily when the eyes are focussed on near objects, gradually dilate. There comes a time when the eyes of the subject no longer change on the approach of the hands of the operator, when the closure of the eyelids by the operator is not followed by an effort to open them, when the subject is thus found to have, to a great extent, lost his voluntary power and to have relapsed into a condition which superficially resembles that of sleep.

This initial sleep of Hypnotism is, however, unlike ordinary sleep, for in the latter the whole nervous system is in a state of subdued activity (thus respiration is slow and shallow), but in the Hypnotic sleep the involuntary

movements determined by the lower portions of the nervous system are exaggerated. A large number of animals are susceptible to the production of this peculiar state.

As early as 1656 Kircher observed that a fowl held with its head on the table, so that the gaze should be fixed on a chalk line drawn in front of it, became motionless after a short time; he called this the *experimentum mirabile.* The humble frog if placed on its back and released, at once turns over into its normal position, but if kept on its back in such a way that all movements are gently suppressed with the head held rigid, it becomes after a few minutes motionless, and remains in this attitude for a little time, a hypnotised frog.

Rabbits, guinea pigs, birds and reptiles can all be brought into the same state by the use of similar methods. In a French poultry-yard refractory hens who persisted in sitting on addled eggs have been profoundly hypnotised by holding their heads under their wings; they have been then placed, when in the state of hypnotic sleep, on a new batch of sound eggs, and, what is more remarkable, have on being wakened at once accepted the responsibilities of the new situation.

The distinction between hypnotic and ordinary sleep is further accentuated by the circumstance that the former change is one which may develop into other striking manifestations of nervous activity. If the closed eyes of a guinea-pig, appropriately hypnotised, are opened and a bright light allowed to flash upon them, the animal does not necessarily wake up and behave like a reasonable guinea pig, instead of doing so it may become cataleptic; all the muscles suddenly stiffen, owing to an uncontrollable rush of energy from the abnormally excitable nervous system. Similar phases of hypnotic existence may occur in man; the resulting muscular contractions being so pronounced that the individual may lie stiff, with merely the head and feet on two chairs, not yielding even when the stoutest member of the audience at the hypnotic *séance* is called up, and seats himself, at the request of the operator, upon the hypnotic's abdomen.

It is interesting to note[1] that cataleptic reflexes can be produced in response to skin stimulation in unhypnotised animals after the separation of the portions of the brain above the cerebellum and spinal cord.

A predominant characteristic of both these phases of the hypnotic state is evidently the paralysis of volitional power, and, as the state progresses, this is succeeded by a remarkable augmentation of other nervous functions. If the eyes have been opened then all the nervous-avenues for the production of sense impressions are present and, in response to peripheral sensory stimulation the nervous mechanism may re-act as it does in the normal waking state, but with two striking differences; first, the actions must follow the sensory impression, they cannot be modified by voluntary control, and any resulting movement of the subject is thus automatic; secondly the sensory impressions adequate to evoke the actions may be far more delicate than those which, in the waking state, are necessary to produce either specific consciousness or the appropriate volitional movement which this consciousness ushers in. The subject does what he is commanded to do; for the nerve processes aroused in ear and eye by the sound of the words, and the gestures of the operator dominate the whole brain machinery of the subject, and a throng of impulses pours out to the appropriate muscles, without the subject being conscious of any stage in the process. It may even happen that the subject does actually remain conscious but impotent; powerless to modify the domincering activity of the nerve mechanism; his actions thus remain automatic although consciousness is present. So, too, in ordinary sleep we may be conscious, for we may dream and often remember our dreams when we wake, and yet we have been powerless to control our actions or to modify the ideas called up by the nerve processes.

This stage, generally an unconscious one, has been termed the somnambulistic stage of hypnotism and is the most interesting for reasons which will appear later. There is however another stage which may be briefly referred to.

[1] Sherrington, *Proc. Roy. Soc.*, 1897; Horsley, *Proc. Roy. Soc.*, 1897.

The physiological derangement, which is the basis of the volitional abeyance and of the exalted nervous activities just described, may, if hypnotism is profound, pass into a more widespread derangement exhibiting itself as deep hypnotic sleep. It is now associated not only with paralysis of will but with profound anæsthesia, and the subject gives all the physiological evidences of lowered vitality of the whole central nervous system. If the state is prolonged it may become dangerous to life by lowering the activities of those lower centres upon the vitality of which such essential processes as respiration, etc., depend. It may, for convenience, be distinguished as the condition of lethargy and resembles that produced by chloroform inhalation.

Proposals have been actually made to employ hypnotism as a method of producing anæsthesia. There are two grave objections to such employment. In order to produce a profound state of hypnotic lethargy, the subject must be made extremely susceptible, and this can only be brought about by repeated hypnotisation. Dr. Esdaile, who employed the method in India, found it necessary to hypnotise his patients every day for several weeks, before he could get them into a state of stupor sufficiently profound to enable him to operate with safety. It is scarcely necessary to point out how utterly unjustifiable such a procedure is; the state itself, when reached, is more dangerous to life than that produced by ether, and far more difficult to be recovered from, whilst, by the processes the subject has gone through, the patient has had those physiological activities upon which his volitional power depends excessively deranged and not improbably permanently enfeebled.

The states thus described—Hypnotic Sleep, Catalepsy, Somnambulism and Lethargy—are undoubtedly due to an abnormal condition of the physiological activities of the central nervous system. In order to suggest what, in accordance with modern physiology, may be presumed to be the *rationale* of this altered condition, it is necessary to sketch the essential features of normal nerve activity. This is correlated with the existence of specific structures and the possession by these of specific functional attributes.

In all higher animals, such as Vertebrates, the nervous system consists of peripheral nerves and central nervous substance, the latter being situated in the bony canal of the spinal column as the spinal cord, and in the skull as the cerebral hemispheres, cerebellum and medulla.

The central masses contain nerve-cells and their branches; these branches may be short or long, some of the long branches form the essential vital portions of the nerves, the so-called nerve-fibres. There is no nerve-fibre which is not the process of a nerve-cell; cut off from this the fibre becomes functionless. It further appears probable that no nerve-fibre is the branch of more than one nerve-cell; each fibre by its course lays down an independent structural path, and although this may divide and subdivide indefinitely it remains distinct. Every nerve-cell with its branches thus forms a complete nervous microcosm, or, as far as the structural path it offers is concerned, a neuron. These may be said to be the two great discoveries of modern neurology, and it is in the light thrown by their realisation that physiological knowledge of the nervous system has to progress. A further fact, the discovery of which preceded those just referred to, is connected with the development of nerve. Nerve-fibres have grown from cells to or from muscles and sensory organs, and innumerable branches within the central nervous system have grown from the cells of one part to and near the cells of a more remote part, both in the spinal cord and the higher cerebral masses.

Sensory or up-lines of nerve communication thus exist from external sense organs to definite portions of the recipient masses; motor or down lines from localised groups of cells in the central mass to muscular and other structures. Between the ends of the up lines in the central nervous mass and the commencement of the down lines in this mass, between the arrival and the departure platforms, is a gap unbridged by such nervous structures. The physiological pecularities due to the existence of these gaps are those to which I especially desire to direct attention.

The more complex the central mass, the more tangled

becomes the skein of long cell processes (nerve-fibres), and of short cell processes; but, just as in the complicated woof of a great loom, so here, the nerve-fibres and short processes, constituting the living threads, remain distinct from each other though woven into close proximity.

Each up nerve-fibre entering the spinal cord divides into two, one branch descending ends near its entry, the other proceeds further to end in various regions nearer the cephalic portions of the system; some of these approach the start of one down line, some that of another, but the gaps always exist between the ending of the one and the commencement of any new line.

It is more difficult to describe, in general language which shall not be too technical, the essential features, not of nerve-structure but of nerve-activity.

The experimental study of the activity of isolated nervous structures has revealed three aspects of their vital phenomena, (1) Living nerves are in a molecular condition which is readily upset by a slight change in their surroundings; (2) they are so constituted that after such an upset they rapidly recover the old position of molecular equilibrium; (3) finally, the altered state due to the upset, although thus rapidly reconstituted, is passed on from the seats of disturbance to such neighbouring portions as lie in immediate structural and physiological continuity with the part that was originally upset. The first aspect, *i.e.*, the capacity to be thus upset is termed nerve-excitability; the second, the upset itself, the excitatory state; the third, the propagation of the upset, is termed nerve-transmission and such transmission constitutes what may, for brevity, be called the passage of a nervous impulse. This nervous impulse is thus compelled to travel along definite routes just as a flame is compelled to travel along a fuse, or a train along a railway line, and this is due to the fact that such travelling is the successive awakening of the line of communication, *i.e.* of portions of definite nerve-fibres. If now the long fuse or nerve-fibre is fired by a local change, in the latter case a change of a sensory end organ, and the nervous impulse thus started travels up the fibre, what may happen when it reaches such

central nervous masses as the spinal cord? The line here divides and the impulse thus scatters; part may go on up to higher portions, but part may travel along the short processes until it reaches their terminations and arrives at the gaps. There it may end, since the line ends; but it may force the gap, struggling through it. In doing this the most obvious fact is the extra time involved in such passage. An impulse takes only $\frac{1}{100}$ sec. to travel up 12 inches of nerve-fibre, it takes, under the most favourable conditions, at least another $\frac{1}{100}$ sec. to struggle across the microscopic gap. For this struggle it may be inadequate and be thus extinguished, but, if it succeeds and reaches the shore of the cell from which the down line starts then it travels rapidly along this to the muscles, and muscular movement must ensue. This is the physiological basis of reflex action indicated in its utmost simplicity and even the most complicated involuntary, that is automatic, movements must involve similar central physiological processes. Two further aspects of these processes remain to be referred to, since they form the basis of those conceptions of the physiological changes in the hypnotic state first put forward by Heidenhain and by Preyer. One of these aspects is associated with the distribution of the structural path; on the ingoing or sensory side the lines divide many times and thus the ingoing impulse is conveyed to many regions; it may thus struggle through gaps in many different situations, and emerge, along various outgoing motor lines, to widely separated organs. This is the physiological expression of the familiar experience that an ingoing nerve-impulse (such as that caused by a prick on the skin) can spread and emerge so as to produce movements of distinct groups of muscles, in arms, legs and larynx. The second aspect is due to the struggle necessary to force a passage through the gap; the physiological path here offers a resistance, hence an ingoing impulse, if weak, may result in no obvious movement. Unable to force the gaps, it has been extinguished at the ends of the ingoing tracts; it has perished in the attempt to cross this unknown region. When once such an impulse has struggled across the gap, nothing can stop its further pas-

sage along the new nerve path which it has entered. It travels along the line thus afforded until it reaches the next gap, when it has to struggle through this. What are the conditions which will be favourable to the forcing of gaps by such impulses? They may be described broadly as of two kinds. One set of conditions is obvious enough: it is associated with the intensity of the impulse itself; the way to produce reflex movements in response to nerve-stimulation is to have such stimulation strong enough. The other set of conditions is more subtle in its causation but equally simple to realise; it is the alteration in the resistance which the gap itself offers to the struggling impulse.

The whole of modern physiology is inexplicable, except on the supposition that the gaps are susceptible of alteration. What this alteration is we do not know; the gap consists of living tissue, and, like all living structures, is constantly undergoing molecular change. It is such ebb and flow which is the essential feature comprised by the term living. It is possible that the alteration is an actual approximation or separation of the terminal cell processes on each side of the gap. But although the nature of the molecular changes causing the alteration are at present doubtful, the physiological agencies by which this can be produced are more definitely known. The resistance of the gap is at the beck and call of nervous impulses proceeding to it along fibres from other portions of the whole nervous system. This is especially the case with nerve impulses which descend to the neighbourhood of the gap from the higher nerve centres. The arrival of such impulses may augment the stability of the molecular arrangements, increase the resistance and thus inhibit or check by this added resistance the passage of the gap by other nerve impulses. It has also been found that the arrival of nerve impulses along other lines or from other higher regions, may, but to a much less degree, decrease the resistance and thus augment the facilities for the passage. Such play, technically termed inhibition and augmentation, or reinforcement, is always going on; the higher centres are always discharging impulses which act in these two ways upon the lower, and the

summed effect is generally inhibition, *i.e.*, increased resistance of the lower gaps. The physiological proofs of this are numberless. I need only cite one: the reflex actions of such an animal as the frog are always aroused with far more ease if the higher centres are not present. The higher centres thus rein in the others, and by paralysing these the reins are cut, the resistance of the gaps is diminished, and they are rendered easier to force.

It is this last phase of central physiological activity which has been assumed by Heidenhain and others to come into play in Hypnotism, and to cause some of those profound alterations in the whole nervous system which are striking characteristics of hypnotic phenomena.

I have already pointed out that the simple method used to hypnotise in man is that of fixed gaze in a slightly strained attitude of vision, necessitating concentrated attention and a prolonged effort of the will. In animals the simple method is forced inactivity and uniform sense impression. A definite view as to the physiological basis of will is beyond modern physiology, but it has been already stated that physiologically it includes capacity to modify the paths of nervous impulses; and it appears probable therefore that the agencies affecting such modification are the play of other impulses upon the resistance of the gaps. It may further be stated that this is probably effected by a specific combination of co-ordinated impulses emerging from the highest parts of the nervous organism, the cerebral hemispheres. Some physiologists (Wundt,[1] for instance) localise these in one portion, the apperception centres of the Frontal Lobes. The process of forced and persistent attention demands continued play of this kind inhibiting some, augmenting other gap resistances in the brain and cord. Physiological investigation shows plainly that continued activity of nerve-fibres is not accompanied by deleterious changes in the nerve-fibres. These are indefatigable, but it is quite otherwise with the structures which surround the endings of nerve-fibres. Here continued activity is deleterious, producing increased resistance to the

[1] Wundt, *Hypnotisme et Suggestion*, 1893.

passage of impulses and the phenomena grouped under the term fatigue. In man the strained and persistent gaze of the subject may result in the production of such fatigue changes in those nerve-endings gaps, the forcing of which is a necessary concomitant of the mental mood of volitional attention; if these become paralysed the volitional power will be in abeyance, and the sleep of Hypnotism ensues. In animals a similar condition is brought about by the forced inactivity and persistent state of constraint. It is this phase of the physiological conditions which Preyer has laid stress upon. The *rationale* of the changes as described above may be considered fanciful by psychologists, but it appears, at any rate, that the resultant effect on the remaining portions of the nervous system is tolerably definite. The particular co-ordination of nervous processes which underlie volition have normally resulted in a varied discharge along nerve-paths to lower centres playing upon gaps, and on the cessation or diminution of such discharges, such regions are released from the inhibitory restraint thus imposed. The other nervous centres are now in a condition to respond with greater ease to the arrival of nervous impulses started in peripheral sense organs; in other words, the gaps are capable of being forced by impulses which would be inadequate were the voluntary power in full development. Such appears to be the condition of the nervous system in the somnambulistic state of hypnotisation with its unconsciousness, will-paralysis, and yet augmented capacity to respond to stimulation of the sense organs.

I am inclined to believe that the increased activity of all nervous processes, except those underlying volition, may be attributed to the diminished resistance of what are here termed the gaps. This itself may be produced, not only by the cessation of the inhibitory influences just described, but by the unrestrained and unmodified flow of such impulses as, by their play, directly diminish the gap-resistance, and thus augment the activity of lower centres. The cataleptic reflexes obtained in the monkey, etc., after removal of the cerebral hemispheres, are probably largely due to the operation of this second factor, which becomes

prominent owing to the cessation of the inhibitory influences. In popular language, the reins by means of which inhibitory restraint was possible having been cut, the influence of the whip has full play. In the case of the somnambulist, the resultant augmentation of nervous centres may be so pronounced as to outlast the state of hypnotic unconsciousness and obvious will-paralysis, so that the subject may show the increased susceptibility to respond to sensory stimuli, even when other evidence of the actual condition of hypnotisation has disappeared. The following experimental data are illustrations of this feature.

Acuteness of Hearing, as tested by Beaunis,[1] was found in several instances to be twice as great in the somnambulistic stage and, in some cases, to remain greater after recovery. The rapidity with which a prearranged movement could be accomplished, in response to a definite sensory stimulus, what is known as central reaction time, was found to be accelerated from 0·3 to 0·2 of a second for hearing and from 0·17 sec. to 0·14 sec. for touch.

I now pass to the final and most subtle aspect of hypnotic phenomena, the part taken in the production of all these phenomena by the train of mental changes known as suggestion. This is in the main a subject which must be dealt with on the psychological side, but its physiological aspects may be conceived along the following lines. We are aware that one idea suggests another and that volitional movements are the outcome of such suggested ideation. The physiological basis for this is decidedly obscure, but modern neurology has comparatively recently brought into prominence one feature which is pertinent to the present inquiry. Functional activity is undoubtedly associated with structural growth, functional inactivity with actual dwindling or atrophy. It is only in the last few years that this has been extended to the processes of the central nervous system. The passage of nervous impulses across gaps is the functional activity of the terminal nerve-fibre branches; if persistently repeated these branches may be conceived as being brought into conditions favourable for their growth,

[1] Beaunis, *Le Somnambulisme.*

tending to approach one another, thus diminishing the actual extent of the gap resistance. With opposite conditions of prolonged inactivity they may tend to recede from each other. Hence the repeated storming of the gaps by nervous impulses would diminish the extent of the gaps and thus facilitate passage across them, provided that such repeated storming is not so persistent as to cause the deleterious changes which constitute fatigue.

The result is that a nerve change of similar type to one which has previously occurred finds its easiest path if it runs along all the old lines; every repetition thus sets up further alterations in localities which are already the seat of similar changes. If the physiological changes involved in such transmission are transmuted into consciousness then ideation follows, but even in the absence of any such transmutation we must assume that a portion of the material basis of the ideation occurs. Such material basis of the idea, a given localised set of changes can obviously occur whether consciousness is present or not, with its entire complexus of discharging nerve impulses. Movements may thus be evoked of precisely similar character to those determined by volition, although both consciousness and volition are absent. Indeed, the absence of consciousness and volition, and the consequent exaltation of all other nervous activities causes such movements to be produced with greater promptitude and exactitude than would be the case if they were present, for now ideas and movements are constrained to follow a preceding set of the mental states. If consciousness is present we are aware of this sequential characteristic and we thus express it by the term "suggested". In the hypnotic state, "suggestion" is used to denote the same state of affairs, although both volition and consciousness are absent.

We are all aware that we fall an easy prey to such suggestion. "As iron sharpeneth iron so a man's countenance sharpeneth that of his friend." We are aware too that this influence is greater in proportion as volitional control and consciousness are in abeyance. How many of us, when changing our clothes in the middle of the day, and

thinking of something else, take out and involuntarily wind up our watches. The physician utilises this power of suggestion when he says to his patient, "You will be better soon". The reaction of the nervous system, in consequence of the suggestion, is one of the most potent of all remedies. But, in a hypnotised person, not only are consciousness and volitional power in abeyance, thus facilitating the sequence, but, in addition, the other nervous activities are augmented; hence we should expect that the power of suggestion would become extreme when by repeated subjection to the hypnotising processes the derangement of the system has become more and more marked.

Further, in such a deranged nervous system the installation of any one of the initial stages of the operations used to produce hypnotisation may obviously entail the whole sequence including even the will-paralysis.

In such people it becomes no longer necessary to fatigue the will by prolonged and concentrated attention in order to hypnotise; the operator may simply start the concentration by arresting the attention and then command the subject to fall into the hypnotic sleep. On the first trial the command will most undoubtedly be futile, but after a few experiences of the hypnotic state the will of many subjects does become paralysed on the word of command, on a gesture, on a look; with frequent repetition of the process, especially in highly sensitive neurotic individuals, a mere casual incident in the chain will arouse the state. The power of suggestion is so great that the Nancy school consider it the determining factor in the production of the Hypnotic state.

Examples are scarcely necessary, but I will refer here to one as illustrating a special point. A patient in the Salpétrière used to be hypnotised at a certain hour. In her normal state she was allowed considerable freedom, and helped in the domestic duties of the establishment. One day she was discovered in a room having opened a chest and taken out some valuables that were not her own. During the theft the clock in the room had struck the hour at which she used to be hypnotised; with her nervous

system highly strung by the fear of detection, she fell a prey to the suggestion started by the sound of the clock and was hypnotised deeply, falling into the cataleptic state on the spot with the things in her hand, to be discovered red-handed some time afterwards, standing a stricken thief. Here it was not the word of command but an accidental accessory, a sensory impression, the potent feature of which was that it suggested hypnotisation because it was an immediate antecedent.

A subject may be thus hypnotised by a verbal command, a gesture, or a written line, even where this is to take effect the next day, or the next week; any one of these initial phases suggests the whole sequence. The hypnotisation of Trilby by the picture of Svengali, described in Du Maurier's novel, is founded on fact. Further the awakening may be achieved in a similar way by suggestion.[1]

It is no wonder, therefore, that such hypnotic subjects should readily respond to sensory impressions, even though these may be far too slight to awaken consciousness in the volitional onlooker. The unconscious subject is an exquisitively sensitive machine with a nervous system tuned to react to impressions of peripheral sense organs, which, acting on the ordinary volitional mortal, awaken no consciousness, and what is more they probably never can awaken the consciousness of such a normal individual since unconsciousness and volitional paralysis are essential factors in making the nervous machinery sufficiently sensitive to respond to the feeble stimuli.

Let me give some instances of such exquisite sensibility, choosing cases in which the series has been started by excessively minute impressions in peripheral sense organs.

1. *From the Skin.*—A hypnotised subject in a dark room is presented with a blank card from a whole series of fifty-two similar ones and allowed to poise it in the hand;

[1] This appears to me to be the answer to such critics of the physiological theories of Heidenhain and Preyer as Moll. His remark that the termination of Hypnosis by suggestion is not consistent with Preyer's fatigue theory of onset is in reality a statement that "suggestion" is inexplicable on this theory.

the card is taken away noted and returned to the pack and the subject being given the whole series, is commanded to choose this special card. The subject selected the card, saying she knew it by its feel and weight.

2. *From the Ear.*—A hypnotised subject was commanded to say when, among several men, a man with a watch entered the next room: the subject did so, saying suddenly that she heard the ticking of the watch in the man's pocket through the wall.

In both these cases there was a possibility of a sense-organ stimulus. The cards were not of equal weight and consistence and the inequality affected the skin of all, but the nervous impulses only forced a passage through the central nervous system of one—that of the hypnotised subject. The sound of the watch ticking must have really affected the ears of all, but the impression was so slight that only the brain of the hypnotised subject responded adequately to it.

3. *Sight.*—Instances of sight are innumerable. A sensitive subject, on being commanded to do so, was able to discriminate with the unassisted eye the details of histological preparations which, under ordinary circumstances, could only be ascertained by the use of a microscope, and to read print, the letters of which were only $\frac{1}{200}$ of an inch long.[1]

The above instances are given as extreme cases illustrative of a very large number of analogous observations, and I refer to them in order to point out to those sceptics who, priding themselves on their common sense, are inclined to scoff, that there is no physiological reason for incredulity, although I fully admit there may be other grounds for suspecting fraud and trickery. The physiological essentials are an exquisitely sensitive nervous mechanism and a distinct initial sensory stimulus; given these two, it is possible, and on the whole probable, that a true physiological sequence should ensue of the same character as that which would be produced by the same stimulus, if it were more pronounced but acting upon a less susceptible machine.

[1] Moll, *Hypnotism*, p. 101.

There is always one feature which is in favour of the truth of these observations; they have been observed in very sensitive somnambulistic subjects; that is to say, the recognition is achieved by people whose nervous system, through the frequent will-paralysis of hypnotism, is precisely in the condition in which it is able to respond to stimuli, which in ordinary life would be quite ineffectual.

The clairvoyant marvels of hypnotic *séances*, telling the number of a bank-note which an operator reads and puts in his waistcoat pocket, and other things of this kind, may, in many cases, possibly in all, be due to collusion and fraud, but there is no absolute physiological reason why they should be. Any person in the presence of such a hypnotised subject is really placed before an exquisitely sensitive machine which can respond to changes in his face, hands, eyes and voice, too small to be noted by either himself or any conscious bystander; it is certain that if such small changes occur they must produce physical phenomena of light, sound, etc.; it is certain that these must stream into and play upon the sense-organs; it is probable, there is no physiological reason against it, that the peripheral sense-organs may be in such a state of poise that they respond to these stimuli even though exquisitely minute, and it is not impossible that, if the terminal organs in eye, ear and skin do respond, the minute messages sent to the brain may produce the same effects as they would do if the person were in ordinary life, and the impulses were the same but much stronger. Further, every investigator may himself, by changes of which he is unconscious, suggest, and suggest unconsciously, to the hypnotised subject an idea which he has in his mind through such agencies as the muscles of expression, etc. The possibilities of suggestion are so extensive that it would be rash to fix a limit to them.

A great gulf, however, exists between all such responses to sensory stimulation, which, however improbable, are physiologically possible, and the doings of the *clairvoyante* who professes to tell not merely a past, but a present which cannot be known by the bystanders, which is happening the other side of the world, or outside the

material world altogether; who holds conversations with disembodied spirits, and is daring enough to project himself into a future which is, from the nature of things, non-existent. There is no known physiological basis even for telepathy, apart from sense-organ impressions, and *ex fortiori* none for such phenomena as the above. It may be that the nervous system will be subsequently found to react to physical changes in the surroundings which are at present unknown, but it is inconceivable that it should do so without such reaction being a response to a definite stimulus.

To a physiologist, with all the details of his science fresh in his mind, every response to an external stimulating agency must be one which involves a peripheral sense-organ and its afferent nerves. If telepathy cannot be explained by means of existing sense-organs, but demands a new sense, then the discovery of such a new sense-organ would alone furnish an adequate physiological basis. To suppose that the processes in one brain can, by a process akin to electrical induction, evoke analogous states in the brain of a distant separate individual is a supposition so opposed to the whole of physiological science as at present constituted that its conceivability demands a mental somersault on the part of the physiologist.

A great gulf thus divides the phenomena due to suggestion from those which are held by superficial observers to be of the same class, but which are in reality widely different, and the evidence for which, is so bound up with cases of imposture and fraud, as to be, in my opinion, unreliable.

I have endeavoured to show that the inhibition theory of Heidenhain and the fatigue theory of Preyer may be extended in the light of modern neurology so as to comprise the phenomena due to suggestion, and thus to afford a physiological hypothesis of the main features of hypnotic phenomena. In this endeavour special stress has been laid upon the part played by the little known substance which intervenes between the terminal processes of different nerve cells. It is not too much to say that the more precise

investigation of the structure and functions of these gaps situated around the termini of cell processes, will form one of the most important of the physiological inquiries of the next decade. Such investigation may be expected to throw a new light upon reflex phenomena, and inferentially upon the states of hypnosis; the object of the present review of the subject is to indicate in popular language the extent to which the physiology of these gaps may be regarded as a determining factor of the essential characteristics of such modes of central nervous activity.

FRANCIS GOTCH.

ARTIFICIAL FLIGHT.

IN a year when specialists in most branches of science are reviewing with jubilation the vast advances which have taken place in every subject of research during the Queen's reign, it is fitting that attention should be turned to the many, and often heroic, attempts that have been made in that period to obtain the mastery, for purposes of locomotion, of that most fickle of all elements, the atmosphere. It is true that hitherto these attempts have not succeeded in solving the problem of artificial flight, but progress in scientific investigation must not be estimated by the actual results achieved, and when the peculiar difficulties of the problem are taken into account, aeronauts will have every reason to be satisfied with the large amount of ground that has been covered during the past sixty years in bringing them nearer to the goal of their ambitions.

A record of the past history of artificial flight takes us back to the legend of Dædalus who as the inventor of sailing ships was not unnaturally accredited with having attached wings to himself. In the fourteenth century, J. B. Dante, a mathematician of Perugia is said to have crossed Lake Trasimene on wings, but further experiments resulted in his breaking a leg. In 1500 Leonardo da Vinci is said to have made some ingenious experiments on flight, and in the seventeenth century a romance was written by one Retif de la Bretonne describing the voyage of a flying man of whom a highly grotesque illustration was given. Passing from fiction to fact, we find in 1678 a French locksmith named Besnier experimenting with a system of double wings, so contrived as to close when raised and to open when depressed.

Besnier made a few experiments; he first jumped off a stool, then he jumped off a table, then he jumped out of a window, and finally he actually succeeded in jumping off the top of a barn; and the *Journal des Savants* tells us that his experiments met with *relative* success (*succès relatif*).

Subsequently he sold the machine to an acrobat, who used it for exhibiting purposes with success.

In 1709, Barthelemy Lourenço, a Portuguese, designed a flying machine, of which illustrations occur in old books. In appearance it was somewhat bird-like, and at either end were two spheres supposed to contain some sort of magic substance, probably either electricity or magnetism, which he imagined endowed with the power of lifting the machine into the air. In addition to these spheres there were a large number of funnels hanging from the top, which upon a windy day would conduct the wind to the under side of an awning and make it act as a sail. If the wind was not blowing, you used a pair of bellows instead, and a man is represented doing this in the figures given in old books. Barthelemy Lourenço wrote to the King of Portugal and submitted the design, asking the King to grant him the sole monopoly of making the machine. The King of Portugal replied that the request was far too modest, and that, in consideration of this wonderful discovery, he appointed Lourenço Professor of Mathematics at the University of Coimbra, at a salary of £3750 a year in French gold. In spite of the royal decree, however, the machine was, of course, never constructed.

An attempt was made in 1742, by the Marquis de Bacqueville to cross the Seine with wings, but unfortunately he broke his leg in addition, and the *Histoire des Ballons* relates that the experiment produced no further results.

Up to this point, it will be noticed, all attempts at aerial navigation had led to negative results. A great advance was made in 1783 by Montgolfier's discovery of the balloon, which opened up a new and highly promising field for research. Henceforth, aeronauts in France became divided into two sections, the *plus leger* party who sought to navigate the air by balloons, and the *plus lourd* who endeavoured to construct a machine which though heavier than air would lift itself after the manner of birds and flying animals. It was natural that the *plus leger* party should for many years hold the field, and we find from the close of

the last century up to the commencement of the Victorian era that balloons occupied a large share of public attention, not only in this country but still more so in France. Many balloonists made large fortunes in those days, and innumerable impossible schemes and fantastic devices were designed to gull the public; among these one of the most grotesque was an aerial vessel called the *Minerva*, with which Robertson proposed to make a tour of the world. The figure given in *Astra Castra* represents a globular balloon surmounted by a gigantic model of a cock bird, and suspended from it was to be a ship in case the balloon should come to grief and the party should have to sail across the sea. Attached by ropes below the ship was an enormous barrel containing provisions for the voyage, and near by a little house was suspended like a bird-cage for any ladies who might wish to accompany the expedition. Quite recently, we believe that a similar project of a balloon carrying a ship for use in emergency has been propounded in an American newspaper.

The earliest convert from the *plus lourd* to the *plus leger* school was the well-known aeronaut Blanchard, who shortly before Montgolfier's discovery had designed a most grotesque flying machine. The aeronaut sat in a chair and by working levers with his arms and legs raised and lowered four large paddles which were supposed to lift the machine. M. Tissandier's quaint figure of the machine is embellished by the addition of a small boy in the stern of the machine blowing a trumpet. The apparatus was designed, but Blanchard expected to get too little recompense for his experiments, and he was going to leave the country when the Abbé Devrunay kept him back and recommended him to take to balloons, which accordingly he did with success. Later on, one Austrian named Jacob Deeghen made a few experiments with a kind of double parachute, which he attempted to work up and down for the purpose of raising himself in the air; but he found that it would not quite lift his weight, so he attached it to a balloon and thus practically joined the ranks of the *plus leger* party. Deeghen took the parachute to Paris and was to have exhibited it there, but

when the French discovered that he was an Austrian they tore his machine to pieces—he himself only escaping after a thrashing.

The difficulty of controlling balloons has from the outset been recognised as the chief obstacle in the way of their adoption as means of locomotion. In the *Histoire des Ballons*, M. Tissandier reproduces an amusing eighteenth century cartoon published in France when ballooning was at its height, representing a *Méthode infaillible de diriger les Ballons*. The figure shows a balloon drawn along by donkeys, the man on the balloon is suspended by his waist and is blowing a horn and carrying a lantern to signal to the donkey-drivers.

In 1850 M. Pétin designed a great air-ship, and actually succeeded in raising over £1000 for its construction. The machine was to be supported by five balloons placed side by side and driven by a gigantic screw propeller. One of the weak points in his scheme was that Pétin said very little about the motive power. His figure, however, shows an engine rather resembling a traction engine at one side of the platform, which would be, one would think, rather a heavy load for the balloons and likely to disturb the balance of the machine. The machine was built, but before Pétin could make an ascent in France he quarrelled with certain officials, and the Prefect refused to give him the necessary permission. Accordingly he went over to England, and then to America, where he had another machine constructed, and made preparations to let it off at New Orleans. The machine was all ready, but it was found that the gasworks were not equal to the occasion, or, at any rate, this was Pétin's excuse for not going up.

Since the time of Pétin the problem of the navigable balloon has been attacked more or less continuously, sometimes, but rarely, with partial success. Among the earlier attempts, a kind of aerial machine was proposed by Lanson, who made experiments with it, but the results were far from conclusive. A system of steering a balloon, invented by M. Hellé, was never experimented with. Another navigable balloon was constructed by M. Dupuy de Lôme,

but the experiments made during the Franco-Prussian war did not lead to any result. Of a fourth contrivance a model was made by M. Julien, who exhibited it at the Hippodrome; it was a small model and would not work. That did not prove M. Julien to be wrong, because the model was on too small a scale to give a fair chance of success. If it had been made sufficiently large it would have worked satisfactorily, so the inventor said.

These old designs appear to us as absurd and grotesque; but Robertson's *Minerva* still survives in various forms, and M. Pétin has his counterpart in the many promoters of syndicates who even now from time to time issue prospectuses of air-ships with first and second class cabins, captain's rooms, engine rooms and saloons.

It is not much more than a year ago that the prospectus of a bicycle company was adorned with a picture of a man in mid-air on a bicycle, quite as fantastic as any of the illustrations in MM. Sircos and Pallier's work, or in *Astra Castra*, and a Toronto newspaper lately gave an extraordinary figure of the "Cowdon Air Navigating Machine" representing a kind of tramcar surmounted by three cigar-shaped tubes bearing the direction "Washington to New York," which tubes, the inventor imagined, or tried to make others imagine, would lift the car. In viewing such designs as these with the cool calculating eye of science, it will be found convenient for the purpose of a rough and ready test, to notice that air is rather over one-thousandth of the density of water, and the weight of the gas contained in a balloon, being somewhere about one-fifteenth of that of the air displaced as relatively small. It follows that a balloon will only support in round figures one-thousandth of the weight that would be supported by a ship of the same displacement. It is thus easy to sift out many worthless designs by imagining the gas bag to be reduced to one-tenth of its linear dimensions and immersed in water; in most cases it will be obvious that such a vessel would be utterly inadequate to support the weight of the machine. Another equally useful test is afforded by remembering that balloons will support about one ounce for every cubic

foot of displacement; in face of which simple fact we find an inventor named Pennington only quite lately patenting a device for seats in an aerial car containing small cavities filled with gas, for the purpose of helping to raise the weight of the passengers, and we have been informed that he, like M. Pétin, has endeavoured to raise money for floating a company to work the scheme.

Continuing our historical retrospect, we learn that in 1854 Captain Le Bris made experiments with a sailing machine which he called an "artificial albatross," and which he released from the top of a cart. Partial success was accomplished, the machine at one time going 600 feet in distance in the air, but the experiments had to be discontinued for want of funds, not, however, till after Le Bris had broken his leg in an accident with the machine. In 1866 there was founded a new society "for the advancement of Aerial Navigation and for observations in Aerology connected therewith". Of this Society, which afterwards became the Aeronautical Society of Great Britain, the first President was the Duke of Argyll, and Mr. F. W. Brearey occupied the post of Honorary Secretary till his death in 1896. In 1868 the Society organised an exhibition at the Crystal Palace, commencing on 25th June. The light motor engines were probably the most important feature, and a prize of £100 was awarded to Mr. Stringfellow, whose steam engine, only weighing thirteen pounds with boiler, gave rather more than one horse-power. About the year 1870 De Villeneuve is reported to have constructed a flying machine driven by steam supplied from a fixed boiler through a flexible hose. On attaching himself to the apparatus and turning the steam on, De Villeneuve, to his surprise, was lifted off the ground, whereupon he stopped the engine in his alarm at the unexpected success of the experiment. In 1871 some experiments on air-resistance were conducted by the Aeronautical Society, a powerful current of air driven through a tube being employed. From that time on, till a few years ago, but few advances have been made, and aerial navigation seems to have undergone a dormant period; the most noteworthy exception being the achieve-

ment of Messrs. Renaud and Krebs of the French war department, who in 1885 constructed a war balloon called "La France," capable of propulsion at a speed of about fourteen miles an hour. Unfortunately, in order to stem most of the winds that blow, a balloon would have to be driven at some forty miles an hour, and herein lies the the great difficulty which has prevented the *plus leger* party from making any great advances. Last year, however, Dr. Wölfert's steerable balloon was shown in working order at the Berlin Industrial Exhibition, and should have given satisfactory results. The gas bag was of oval shape, nearly 100 feet long and 30 feet in diameter, and there were two propellers, one about 9 feet in diameter in front of the car acting horizontally, and another placed underneath to raise and lower the machine. These propellers were to be driven by an eight horse-power[1] benzine engine making 500 revolutions per minute. Unfortunately, at a trial trip on 12th June, the baloon exploded and burst into flames, and both Dr. Wölfert and his assistant were killed.

A new era was opened by the *renaissance* of the *plus lourd* party which took place about the year 1891, with the advent of an army of workers, including the names of Maxim, Lilienthal, Langley, Pilcher, Chanute, Baden-Powell, Phillips and others. All these are now familiar names, and their apparatus and experiments have been so widely figured and described by the Press that a mere description of them here would hardly be of interest. To understand their true significance, however, it is necessary to consider one by one the various qualifications which are required to make up a successful flying machine, and how they have been attained.

The problem of artificial flight will practically be solved when a machine has been built which will raise itself and a man off the ground into free air, which can be propelled and guided for a certain distance in any desired direction, even against a moderate wind, without fear of its breaking or overturning, and which, with the experimenter, can be

[1] *Aeronautical Journal, January*, 1891.

brought safely to rest on the ground at the termination of the flight. The principal requisites are:—

(i.) A sufficiently powerful and light motor.
(ii.) Propellers, and aeroplanes of sufficient lifting power.
(iii.) Balance and stability.
(iv.) Appliances for steering and controlling the machine.
(v.) Strength and rigidity.
(vi.) Means of landing safely.

(i.) *The motor.*—The difficulties of flight at the beginning of the Queen's reign were entirely different to and far more insuperable than those of to-day. Up till quite recently no experiments on artificial flight could possibly have led to any practical successes owing to the want of light powerful engines or motors. The great advance made in late years in this direction, and which may be expected to be followed by further and greater advances in connection with autocars, has already entirely modified the conditions of the problem, and will probably continue to modify them still further in the future.

The early experimenters had no motive power to depend on beyond that of their own muscles, and we now know that man does not possess sufficient power in proportion to his weight to maintain himself in the air. He can start from the top of a hill and sail downwards through the air under gravity, and his rate of descent furnishes a simple if not quite accurate test of the power he would have to exert in order to fly. For if, instead of sailing in a downward path, a man wanted to fly horizontally, *he would require to exert about as much horse-power as would enable him to run up the hill carrying the flying machine with him, in the time that the flying machine takes to descend to the bottom.* Now Mr. Pilcher finds his rate of descent to be about five feet per second under favourable circumstances, and hence it follows that a two horse-power engine would be necessary to keep him afloat.

In discussing this point further, I cannot do better than quote from the data collected by Mr. Jeremiah Head in his

able address to the British Association at Nottingham. Dr. Haughton reckons that men rowing in a boat race exert about one quarter of a horse-power, and if we take a man weighing about twelve stone or 168 pounds, it follows that he weighs 672 pounds per horse-power that he is able to exert. Mr. Maxim, on the other hand, has proved that heavy birds only weigh 150 pounds per horse-power that they are capable of exerting; so that they possess about four times the horse-power per pound that we have. The albatross and the vulture probably go up to 250 pounds per horse-power. It thus appears impossible for man to fly by his own exertions.

But we are now able to construct machines which will develop a far greater horse-power in proportion to their weight than any bird. Ten years ago Mr. Thornycroft built a large torpedo-boat (the *Ariete*) which exerted one horse-power for every 191 pounds dead weight. This is less weight per horse-power than some birds possess; but we are not justified in concluding that the vessel could fly if it were furnished with wings, for horse-power per pound or weight per horse-power is not the *only* factor on which the possibility of flight depends. It does not follow that when the size of a bird or of a flying machine is increased the horse-power is directly proportional to the weight. That law of proportionality in fact we know fairly well does not hold. The horse-power must in fact increase more rapidly than it would do if merely proportional to the weight. Assuming that the resistance of the air is proportional to the superficial area and to the square of the velocity, and that flying animals of different sizes are similarly proportioned, according to M. de Lucy's law (which is roughly verified for birds and insects), a very simple piece of calculation by elementary mechanics will show that if all the dimensions of such an animal were quadrupled it would have to exert double the horse-power per pound, or 128 times the actual total horse-power, to sustain itself in flight. Under similar assumptions large flying machines would require greater horse-power per pound than small ones, the general law being that the horse-power per pound is propor-

tional to the square root of the linear dimensions. If, then, a large machine can be made to lift itself off the ground, a smaller machine could be more readily made to do so, and this circumstance adds weight to the experiments conducted on a large scale which have brought Mr. Hiram S. Maxim into prominence as an authority on aeronautical matters.

Maxim used steam power, the fuel used in the furnace being naphtha. This was burned in a very large number of burners (700 or more), so arranged as to give the largest possible area of flame. The steam was generated in a water-tube boiler similar to those now used on fast ships. It consisted of an enormously large number of fine tubes, giving the largest production of steam in the smallest space, while as for the engines they were so light that Maxim easily lifted them in his hands. The total weight of his machine was about 8000 pounds, and he reckoned that it did not weigh more than eleven pounds per horse-power. Seeing that birds may weigh 150 to 200 pounds per horse-power, it will be admitted that Maxim had sufficient motive power in proportion to weight; and, what is more conclusive, he has actually been able to make his machine lift itself from the ground when travelling at about thirty-seven miles an hour.

In order that there might be no danger of accident it ran on a railway, and outside there was an outer railway, so that as soon as it rose a few inches from the ground it pressed upwards on the outer rails, and should thus have been prevented from becoming uncontrollable. Once, however, when the engine was developing 362 horse-power, the outer rails gave way, and the machine actually did fly. That flight was a very expensive one, as it cost nearly £1000 to repair the machine, but from a scientific point of view it formed the crowning point of Maxim's experiments; for after the accident the rails were all torn up, affording proof positive that the machine possessed sufficient power in proportion to its weight not only to lift it into the air, but to bend the bars by which the outer wheels were attached, and to pull up the rails as well.

Of course, on the other hand, the construction of an

engine weighing eleven pounds per horse-power is a different matter to the construction of a one-horse-power engine weighing eleven pounds; and as the primary object of all experiments is to obtain a machine that will carry the weight of a man, there are certain limits of size below which machines would not be of practical use. It may be noted, too, that the *steam turbine*, owing to its absence of vibration, promises to be a very useful and suitable motor for the purposes now considered, and may in all likelihood supersede engines with reciprocating parts.

(ii.) *Propellers and aeroplanes.*—Flight like all other forms of locomotion depends on the principle of action and reaction, but there is this important difference, that whereas in a rowing boat the reaction of the water on the blades of the oar only has to move the boat forward, in a bird or flying machine the reaction of the air set in motion by the wings or aeroplane has to continually overcome gravity. To keep a body heavier than air afloat in the atmosphere it is necessary that downward momentum should be continually communicated to the surrounding air at a rate proportional to and measured by the weight of the body. This communication of momentum involves the continuous expenditure of work, but this work is measured by the kinetic energy and not the momentum imparted to the air. This kinetic energy for given momentum is inversely proportional to the mass of air acted on, hence in order that there may be as little waste of power as possible in supporting an aerial machine it must be provided with aeroplanes acting on as large a mass of air as possible. Now there are two ways of acting on a great mass of air; one is by constructing enormously large aeroplanes, the other is by imparting a considerable horizontal motion to the planes. For if we move a plane rapidly through the air in a horizontal direction it will constantly come in contact with fresh particles of air in succession, and it will thus be supported by a far larger mass of air than it would if it were to constantly remain in the same place, simply on account of the rate at which it moves. This property is not unknown to engineers; the ordinary screw propeller of a ship affords another illus-

tration. If a ship is at rest in port and the screw is set in motion, very slight force is sufficient to keep the ship from moving, for the screw only keeps churning the same portions of water, and so has little power; but as soon as the ship is under way the propeller is continually brought into contact with fresh portions of water previously at rest, and so gets a much greater grip on the water than when the ship was at rest in the harbour. But for this property it would be impossible to drive large ships forward with such small propellers as are commonly used. And just the same applies to aeroplanes.

These simple conclusions from elementary principles were verified in a striking manner by Professor Langley and Mr. Maxim, and the former has embodied the results of a long series of observations in his *Experiments on Aerodynamics*, which is now regarded as a standard work of reference. Mr. Maxim made similar investigations quite independently at the same time, a whirling table being used by both observers. The method was this: a plane was made to revolve round a central axis and the force of pressure of the air on it carefully measured so as to discover the laws according to which the resistance of the air varied when the velocity or the inclination of the plane was varied.

The first conclusion that these experiments brought out was that if a plane be held perfectly horizontally it falls to the ground less quickly if it has a horizontal motion imparted to it (as when made to travel round and round by means of this whirling table) than it would do if it were simply falling vertically. In other words, the horizontal velocity lengthens the time of falling. The curves obtained indicate that for small speeds the effect is not very great, but for speeds exceeding about thirty miles an hour the lengthening of the time of fall increases very rapidly.

The dropping plane, it will be seen, imparts a little downward motion to the air as it drops, but as soon as it has gone a little distance it comes in contact with fresh particles of air at rest; it is never supported for any length of time by particles to which it has already imparted a per-

ceptible downward velocity; it is, therefore, constantly striking air *at rest*, and accordingly is much more retarded than it would be by making its way constantly through the air that it has set in motion.

The second result which Professor Langley and Mr. Maxim's experiments proved was this: the horse-power required to sustain a given weight in horizontal flight by means of an aeroplane of given dimensions is less for high speeds than for low ones. In other words, the faster the aeroplane travels, the less energy is required to keep it up in the air for the *same* length of time. This fact doubtless accounts for the high speeds at which birds fly, and it depends upon the same principle as the first result, namely, that the more rapidly the plane moves, the more it comes into contact with fresh particles of air at rest.

As regards the variation of the air pressure according to the inclination of the plane the general result of Professor Langley's observations was to confirm the empirical formula previously enunciated by Duchemin.

From the experiments of Professor Langley, it also appears that a rectangular aeroplane moving perpendicularly to its longest side experiences greater support from the air than it would do if it were moving in a direction parallel to its longest side. A flying machine should therefore imitate the shape of a bird—with narrow wings standing out at right angles to the direction of motion rather than that of a fish. This result is easily explained on the theory that in a wide plane the particles of air coming in contact with the hinder portion have already been set in motion by the foremost portion, and consequently less reaction can be obtained from them. A further verification of this explanation is afforded by Professor Langley's observations of the position of the centre of pressure, which in all cases is in front of the middle point of the rectangle, showing that the pressure is greatest on the foremost portions.

The advantages of narrow over wide planes have been most conclusively shown by Mr. Horatio Phillips in some experiments conducted at Wealdstone near Harrow a couple of years ago. "Phillips's Flying Machine," as it

was called, resembled a Venetian blind in structure, being constructed of a number of laths at a small distance one above the other and driven by an engine working a propeller. Although the machine was never allowed to fly, being constantly held attached to a whirling table, it rose from the little circular track on which it ran, and the lifting power of the planes was found to be considerably greater than that of a single wide plane of the same area. Phillips had made elaborate experiments on wide planes before coming to his present conclusions.

Although there are some difficulties in adopting the Venetian blind form to "soaring" experiments, the advantages arising from the use of narrow superposed planes are generally admitted by engineers, and they have been introduced in modified forms with considerable success.

Where however the wing surface is required to be of any considerable width from back to front, curved surfaces are better than plane ones, as I now hope to explain.

When an inclined plane surface is moving in a horizontal direction or is acted on by a horizontal wind, the total pressure is perpendicular to the surface, and by the elementary laws of mechanics we can resolve this force into two components, *viz.*, an upward force, called the *lift* (which *lifts* or supports the weight of the plane), and a horizontal force called the *drift* (which represents the force with which the plane must be *driven*). By decreasing the angle of inclination of the plane to the horizon, we decrease the drift, but if the plane is *perfectly* horizontal, the wind will exert no pressure on it, so there will be no lift or drift at all. If, however, instead of using a plane surface, a concave curved surface of the same area is experimented with, it is found that greater lift and less drift are obtained, so that it is possible to support the same weight with less expenditure of power than before. This result holds good provided that the curved surface is inclined at a moderately small angle to the horizon. When its inclination to the horizon is considerable, the reverse is the case; there is greater drift and less lift than with the plane surface. But this is exactly what is wanted; for in order to stop a flying

machine it is necessary to tilt the aeroplane more nearly vertical, after the manner of a bird coming to rest. We want to check the horizontal speed as much as possible, accordingly the drift must be as great as possible, and here the concave surface is again the best.

Although most practicable designs for flying machines involve the use of the screw as a means of propulsion, Mr. Hargrave has constructed models working with flapping wings and finds little difference between the efficiency of this means of propulsion and a screw. But even here fixed aeroplanes were used in addition, and there can be little doubt that the ultimate form of the flying machine will be one supported on fixed planes driven through the air at high speeds. Several fanciful schemes for constructing gigantic air ships propelled by other means have been put on paper and have even lately appeared in print, but these must be viewed with suspicion. It remains to add[1] that Professor Langley and Mr. Maxim with small slanting planes have been able to support about 250 pounds per horse-power, exerted, and the latter, by using heavier weights, about 133 pounds per horse-power. Both of these figures, taken in conjunction with previous results, show that, so far as purely mechanical difficulties are concerned, the problem of flight is soluble.

(iii.) *Balance and stability.*—If it is of fundamental importance that a ship should be properly balanced and should not be liable to overturn, the same applies with even greater force to an aerial machine; but the conditions to be satisfied in the latter are more difficult of investigation, and failure in this respect is attended with greater and more certain disasters in mid-air than on water. In his lecture to the British Association at Liverpool last year, Dr. Elgar pointed out that the stability of ships was not investigated mathematically till after many vessels had sunk, and that our present knowledge was only acquired as the result of considerable sacrifice of human life. The sad ending which befel Herr Lilienthal just when his experiments were beginning to be attended with their greatest success is the most unfortunate

[1] *British Association Nottingham Report*, p. 872.

one of a series of catastrophes which make it probable that we may pay as dearly for conquering the air as we have done in the past for ruling the waves.

Probably no experiments have contributed so largely to our knowledge of the conditions of balance and stability of aerial machines as those initiated by Lilienthal, and which have been brought to this sad termination. His work fortunately survives, and among his disciples there is no keener aeronaut than Mr. Percy S. Pilcher. On the other side of the Atlantic much valuable work has been done in the same direction by Mr. Octave Chanute, a former President of the American Society of Civil Engineers.

Remembering that the symmetrical form of all aerial machines experimented on is sufficient to secure what might be called their "transverse balance," the necessary conditions are really threefold, viz., transverse stability, longitudinal balance and longitudinal stability. It will, however, be convenient to deal with the two last together.

Transverse stability, or stability for rolling motions, is the only kind of stability which seriously affects the naval architect, who knows that it can be secured, so far as small displacements are concerned, by making a certain point called the metacentre—whose position he can calculate mathematically—above the centre of gravity of the ship. In an aerial machine various factors affect the corresponding kind of stability.

According to our ordinary statical notions the stability of any body in equilibrium can be increased by lowering its centre of gravity. It might thus appear at first sight that a soaring machine would be made more stable by bending the wings upwards at the tips, by raising the aeroplane, or by suspending a heavy weight considerably below the machine. But every shipbuilder knows that if a vessel is to sail steadily its centre of gravity must not be too far below its metacentre, otherwise the ship will roll from side to side in a heavy sea to a very inconvenient and even dangerous extent. So our ships are built with only a very small metacentric height; so small indeed that if the centres of gravity were raised a foot or so higher than their actual

positions the ships would become top heavy and overturn sideways. It remained for Pilcher to prove that a similar condition is equally essential in the case of aerial machines.

Pilcher's first machine had the wings very much upturned, and was provided with only a vertical rudder, not a horizontal one. But it was found that the machine pitched too much, so that it became rather dangerous to experiment on, and Pilcher then added a horizontal rudder which made the balance much better. He found, however, that although the V shape of the wings made the machine very steady when sailing against a head wind, a side wind was apt to tip it over, so he had another machine made with the tips of the wings flat instead of being curved up, but the wings were now placed at nearly the height of his head. Here again it was very difficult to balance with the weight so far below the wing surface, accordingly Pilcher took his first machine and bent the wings quite flat at the tips. He now found that by having the wings so much lower down he could balance much more easily; of course, if the weight were *too* high up, the equilibrium would be unstable; but Pilcher's experiments show that for safety the statical measure of stability must be small. On the modified machine form, Pilcher could sustain himself in the air for several seconds, and he was sometimes picked up by the wind and lifted as much as twenty feet above the ground; sometimes, also, he got some one to pull him along by means of a string, like a kite, or to hold him against the wind, and quite a moderate force was sufficient to hold the machine in mid-air. One of his achievements last Easter was to descend eighty yards in a calm, and noticing a large bush in the way he actually made the machine rise up in the air so as just to clear the obstacle. And on 19th June, he glided for over 250 yards in the presence of a number of spectators, an experiment which will be described in *Nature*, probably before the present article is in print.

Pilcher is hoping to get a small petroleum motor attached in order to make the machine self-propelling and thus convert a mere glider into a true flying machine. For an

eleven stone man, he says that no soaring machine should weigh more than sixty pounds, because it fags one out too soon, and the strain brought on is too severe. Many people seem to think that a machine can be fully tried in a few hours; as a matter of fact it is only after having handled the apparatus for several long days for hours at a time that one begins to understand it at all.

The arrangement of two superposed planes used by Lilienthal in his latest experiments has not been adopted by Pilcher, although he took several sails from the top of Lilienthal's hill on the machine shortly before the death of its inventor; the chief objection raised by Pilcher to the double surface being its unsteadiness in windy weather caused by the height of the upper surface—in short, its too great statical stability.

Longitudinal balance and stability.—Although it is essential for a ship to be properly balanced, the shifting of its centre of gravity slightly fore or aft would only cause the vessel to sink at the bows or stern and, even if the effect were considerable, it could easily be corrected before it led to any disastrous consequences. In an aerial machine, this longitudinal balance is much more difficult to deal with, since shifting the centre of gravity forwards causes the machine to dive downwards, and shifting it backwards causes the planes to tilt upwards and the apparatus to lose headway, become uncontrollable, and perhaps fall over backwards.

These facts, besides many others, can be verified by any one with the use of one or two gliders made of paper. It is only necessary to fold a sheet of paper down the middle so as to make it slightly V shaped and to load it with a small piece of stick to see how difficult this balancing is. Setting aside the fact that gliders have an obstinate habit of turning on one side instead of flying straight, the experimenter will probably lose patience before the glider can be balanced so that it will sail down steadily at an acute angle with the horizon.

But even after the glider has been properly balanced a certain amount of skill is necessary in projecting it at the

proper angle and with the proper speed in order to make it fly well, and, let it be remembered, every failure would represent a fatal accident on a flying machine. Instead of sailing uniformly, the glider will, as a rule, if fairly well thrown, describe a series of undulations in which, as in waves on water, the crests are narrower than the troughs: in short, curves much resembling a trochoid. To a mathematician it is a trifling transition from these crests to cusps, and from cusps to loops, and both of these forms are represented in the paths described by gliders, but the glider very often comes to rest and then falls down backwards instead of turning over and over. The general form of all the curves can be easily explained by the variations of the pressure with the relative velocity of the wind, and of the position of the centre of pressure with the inclination; and I would suggest that a series of instantaneous photographs of the path of a glider, from which the forces and couples acting on it might be calculated, would afford an interesting means of verifying the laws of aerial resistance, just as the path of a planet affords proof of the law of gravitation.

The question next arises: Do the undulations remain constant in shape as the glider descends, or do the crests become more pointed till they become loops, or does the reverse take place? If the second happens, the glider is longitudinally unstable and dangerous, while if the third happens it is longitudinally stable and safe. If the first happened the motion under proper initial circumstances would be stable but periodic, and any puff of wind might alter its character and cause the glider to upset, and moreover, if started from rest (*i.e.*, without initial velocity) the glider would certainly upset when it again came to rest. It is easy to build up an explanation of Lilienthal's fatal accident from some such considerations as these; seeing that it is known that he actually was brought to rest by the wind, and in trying to get way on by tilting the machine forward, he fell right over and was killed. It is quite possible that had Lilienthal's machine carried a motor capable of restarting its forward motion at the critical moment, he might have been saved.

I know of no other mechanism in which this difficulty of longitudinal stability occurs except the old fashioned "Otto bicycle" where the rider sat between two large wheels, and the analogy suggests that the manipulation of a gliding machine is probably about as difficult to learn as riding *on one wheel.* This problem of longitudinal stability of aeroplanes does not appear to have been worked out mathematically. Experience has shown that a single surface of considerable fore and aft length is very unstable, and the narrow planes which are best for lifting are much better, but it is essential for stability that either a horizontal rudder or tail should be attached, or two or more planes should be placed one behind the other.

It is clear that if a change of inclination shifted the centre of pressure of a plane two feet wide through a distance of two inches, the shift for two separate planes each one foot wide would be only one inch, and consequently the combination would sail much more steadily, a result which any one can verify by experimenting with gliders. As regards the tail, the steadying action of birds' tails is sufficiently known, but it may be worth while to call attention to the fact that one of the most skilful of European butterflies in gliding flight, is the long-tailed swallow-tail (*Papilio podalirius*). The practical necessity of a tail in experiments on artificial flight was discovered by Lilienthal.

While birds are able to control their longitudinal balance by slightly turning their wings Lilienthal and Pilcher, experimenting with fixed wings, have been obliged to move their weight about in the machine, for which purpose they have supported themselves by their elbows only, leaving their legs free for the violent exertion required in tussling with a gusty wind.

Mr. Octave Chanute's experiments have, we believe, been directed towards obtaining a large machine that will glide steadily by itself without the performance of these gymnastic feats, and he considers that until this can be done, but little good can result from experimenting with motors. A number of successful glides have been made by him, many of them of considerable length. The successes

achieved by Professor Langley, which have been chronicled by the press, have consisted chiefly in the construction of a machine, driven by a motor, which has kept its balance continuously for a long flight. His model is not large enough to carry a man, and in the figures which appeared in the American papers, the addition of a person standing in the bows must be regarded as artistic licence.

(iv.) *Steering arrangements* need but little notice here. Lilienthal and Pilcher and other experimenters on gliding flight have acquired considerable skill in guiding their machines by balancing alone, and it is noteworthy that Lilienthal's fatal experiment was his first attempt at using a movable rudder. If aerial machines should ever be constructed on a large scale balancing will, of course, be out of the question, and horizontal and vertical rudders of some kind will be necessary, and it may be advisable that they should be experimented with soon.

(v.) *Strength and rigidity*, unlike the last conditions, are qualities with which all engineers are familiar. In the case of a flying machine this factor introduces peculiar difficulties as Maxim was not slow to discover. In order to keep a flat surface of considerable extent rigid, numerous struts and stays are required, and these Maxim finds add enormously to the resistance of the air at such speeds as are required for flight. Curved surfaces, as every mathematician knows, are better in this respect than plane ones. The only way of overcoming the difficulty seems to be to make the machine as small and compact as possible, and here the advantage of superposed planes becomes evident. Thus the conditions of rigidity are in no way antagonistic to the other conditions before enumerated. It is evident on the other hand that want of rigidity, by allowing the aeroplanes to bend, might considerably modify the stability of the machine.

In this connection I would call attention to the recent progress in kite flying since the subject was first taken up scientifically by Captain Baden-Powell in this country, Professor Hargrave in New South Wales, and others, which has resulted in the discovery that a box with open ends forms a most efficient kite, possessing far greater rigidity

than the old quadrilateral of our playgrounds, and, further, that the combination of two such boxes connected by horizontal bars, flies as a kite remarkably steadily and stably. Such matters as strength and *rigidity* can well be investigated by experiments with kites, which can be conducted at small cost and with little risk, while other more difficult investigations are being pursued by the students of flight proper. Moreover the old taunt of the French *plus leger* aeronauts that the *plus lourd* had never succeeded in ascending in the air has been completely met by Captain Baden-Powell's experiments in which people have ascended to considerable heights and descended in safety by means of kites.

(vi.) *Safe landing* has also been successfully accomplished by Lilienthal, Pilcher and others. It is most difficult to learn to land with a horizontal speed on. When beginning, one is certain constantly to tumble over forwards; the only way to stop is to get back in the machine and tilt it up in front so as to cheek the forward movement, thereby imitating the action of birds as revealed by M. Marey's photographs, described in his *Vol des Oiseaux*. Of course this is very difficult at first.

In order that this feat of landing safely may be accomplished, the machine must possess an aeroplane of large extent, such as will act as a parachute and encounter the greatest possible resistance. Another feature is that the machine must not be too large a one to be controlled by balancing. The stopping of a machine propelled by a screw presents difficulties. If the screw were reversed, the machine would simply be driven down on the ground. If the screw were driven forwards, it would have its horizontal motion increased instead of retarded. Pilcher proposes in such cases to stop the action of the propeller altogether, and rely on the aeroplane to stop him. The designers of large air ships are, as a rule, reticent on the means to be adopted for bringing them to earth.

Summary.—Now it will be observed that the conditions for a good flying machine, like the conditions of sensitiveness and stability of the common balance of our text-books, are to a certain extent incompatible. Every one of these conditions has been fairly satisfactorily dealt with by some

experimenters; it only remains to embody them all in a single machine.

In what direction is the solution of the problem to be sought? If any experimenter can so thoroughly master the control of a machine sailing down-hill under gravity as to increase the size of the machine and make it large enough to carry a light motor, and if, further, this motor can be made of sufficient horse-power, combined with lightness, to convert a downward into a horizontal or upward motion, the problem of flight will be solved. The first flights need not be long—a hundred yards, rising, say, twenty or thirty feet above the ground, will be sufficient; all else will be simply a matter of improving on the original model, and once success is assured workers will not be wanting.

Another promising direction for success lies in an elaborate and exhaustive investigation of balance and stability, such as would allow of the safe use of motor-driven machines too large to be controlled by mere athletic agility. This might partially be acquired by experiments with models, gradually increasing in size till they were capable of carrying a man and motor. But if the future development of artificial flight is not to continue a repetition of the chapter of accidents by which naval architects gained their theoretical knowledge, there is abundant work for mathematicians in reducing the conditions of stability of aerial machines to a matter of pure calculation. One thing is certain, till this is done designs of large air-ships for the wholesale transport of passengers, officers and cargo are not of the slightest value; their designers would do better to study mathematics and help in the heavy work of calculation still requiring to be done.

Very different from the *air-ship* is the *flying-machine* by which it is sought to commence with small feats and gradually work up to big ones, and considering the number of workers at present in the field, and the scientific and systematic way in which they are studying the difficulties of their problem, we may yet hope that the realisation of artificial flight will take its rank among the scientific triumphs of the Victorian Reign.

G. H. Bryan.

ON PROGRESS IN THE STUDY OF VARIATION.

THAT variation is a subject worthy of study for its own sake is becoming recognised, and the numbers of naturalists who are devoting themselves to this study steadily increases. Probably, however, if the nature of the observations required were more generally understood progress would be much more rapid.

The first object of the study is to throw light on the origin of species. An exact knowledge of the facts of variation concerns many departments of biology, but it is in their bearing on the origin and nature of specific distinctness that these facts have their most direct and most manifest importance. It is, then, essential that the subjects chosen for investigation should be such as may be supposed to have this bearing.

Elsewhere (2) I have argued that it is reasonable to suppose that variations of a discontinuous nature may play a preponderating part in the constitution of new species, inasmuch as individuals presenting discontinuous variation in any character differ already from the normal of their own species—in respect of that character—much as one species differs from another.

For various reasons many have taken exception to this suggestion, and though I do not now propose to enter with any detail into matters of theory, nevertheless it may be well to note the fallacy of one line of reasoning upon which objectors have especially relied. It is said for example that discontinuous variation is an exceptional phenomenon, that "sports" are so rare that they may be neglected, and that the appearance of discontinuous variation often seen in collected series of specimens is due to an arbitrary system of collection. These objections are founded partly on an imperfect understanding of the suggestion that discontinuous variation may be a chief factor in evolution, but far more often they proceed from a want of familiarity with the class of facts upon which the suggestion is based.

First, it is evident that there is considerable misapprehension as to the sense in which the term discontinuity is used in its application to variation. Discontinuous variation is carelessly supposed by some to be synonymous with "sport". While however a sport is a discontinuous variation, it is not in this restricted sense that the term should be used. The word "sport" is generally used to denote a variation connected with the normal by no intermediate individuals. In many—perhaps in most—cases of discontinuous variation there is a complete series of intergrading individuals, not necessarily in each direct line of descent, but occurring among the varying population. Discontinuity in variation is a term of degree. A variation is discontinuous *in proportion as* intermediates between the variety and the type are comparatively infrequent. For a variation is discontinuous if, when all the individuals of the population are breeding freely together, there is not simple regression to one mean form, but a sensible preponderance of the variety over the intermediates. When such a population is seriated in respect of the varying character for statistical study in the manner with which naturalists have been familiarised by the writings of Galton and others the curve of variation has not one peak as in a monomorphic species, but has at least two peaks. The essential feature of a discontinuous variation is therefore that, be the cause what it may, there is not complete blending between variety and type. The variety persists and is not "swamped by intercrossing".

While the word "sport" is expressly used to denote something rare or monstrous, no such idea necessarily attaches to discontinuous variation. When in a species commonly known to us as a constant and comparatively invariable form, an individual is found showing a striking variation, we call it a sport. Thus the six-rayed *Sarsia* ((2), p. 424) is a sport. But in some Medusæ the presence of individuals exhibiting similar meristic variations, either regular or irregular, is so frequent that these "sports" are a normal constituent in any sample.[1] If in England a speci-

[1] For example *Aurelia*, see Browne, Q. J. M. S., xxviii., p. 245; various authors *Nature*, 1894; *Zool. Cblt.*, ii. p. 37, etc., and *Eucope*, Agassiz and Woodworth, *Harv. Bull.*, *Nov.*, 1896, p. 121.

men of *Argynnis adippe* were captured having the silver spots on the underside of the hind wing absent we should call it an extraordinary sport; but in most subalpine localities such specimens are common both in *A. adippe* and *A. niobe*. In the latter species in fact it is the silver-spotted form which is exceptional in many localities.

In any extended view of variation it is soon apparent that a variety which in one species and one locality is exceptional, may in another species or another locality be of constant occurrence.

So also in regard to the reiterated assertion that the appearance of discontinuity between type and variety is due to the arbitrary rejection of intermediate forms. It is true enough that in very few cases are proper statistics forthcoming, and that as yet it is impossible to say *how much* discontinuity exists in any but a few examples. Nevertheless the fact that in innumerable cases the type is common, and a variety is common relatively to the intermediate forms is familiarly known to collectors. Everything depends on the species and the variety chosen for illustration. For example, in the moth *Callimorpha hera* the hind wings are generally red, but a yellow variety is common. In this species it happens that specimens of intermediate tints shading through orange and buff are frequent. So far as I know it has not been established statistically that the intermediates are less common than the yellow variety, and possibly enough they are more common. But in *Thais medesicaste* the discontinuity between the type and the variety *honoratii* found flying with it at Digne, in which the yellow of several cells wings is replaced by red, cannot be questioned. The intermediate forms are either absent or quite exceptional, though type and variety occur together.[1]

The further we proceed in the study of variation the clearer it becomes that we have no warrant for applying the conclusions as to variation and inheritance derived from the

[1] In illustration of the want of blending in this case it may be mentioned that a specimen was reared by Standfuss ((8), p. 319) which was *honoratii* on the right side and *medesicaste* on the left.

examination of one species or one character to the problems presented by other species or other characters. The properties of species are in brief *specific*, as the properties of chemical substances are; and as the properties of each chemical substance have to be separately determined, so must the specific properties of each species be the subject of separate and special study. In what may be called the Chemistry of Species we are far indeed from the era of generalisation.

Discontinuity in variation is in great numbers of cases an observed fact, while the supposition that when organisms are freely breeding together in the state of nature all the varieties they display in size, colour, structure, and so forth, are capable of freely blending, and that the offspring of dissimilar parents tend always to regress to one mean form is not borne out by the facts. Whether continuity or discontinuity is found depends on the species studied and the character selected for investigation. There is continuous variation, but there is discontinuous variation also. To discover by statistical investigation the degree of continuity or discontinuity which in each species is manifested by the variation of each character is the first business of the student of evolution.

Those who do not admit the probability that discontinuous variation may have a preponderating influence in the establishment of new species will remember that this is confessedly a matter for individual judgment, and whatever views may be entertained on this matter the fact that discontinuity is frequent in variation remains indisputable. Indeed, if it is admitted that the problem of evolution is to be attacked by the study of variation no assumption need be made to justify the prerogative claims of cases in which there is an appearance of discontinuity; for discontinuous varieties have, *prima facie*, already something of specific distinctness, and whether they are or are not those nascent species we are all seeking, they at least look more like them than other varieties do.

We are in a preliminary stage, and apart even from any question of continuity or discontinuity it is surely well that

those who are contemplating systematic investigation of the problem of species should begin by the study of such cases as these. It is, in short, reasonable that the study of variation should be begun with the systematic study of varieties.

By disregard of this limitation a greater scope for statistical refinement may be rendered possible—for the observer is thus free to choose the subject most amenable to such treatment—but it may be questioned whether the compensation is adequate, and whether the solution of the particular problem of the nature and origin of specific differences is greatly promoted thereby.

With this limitation, however, begins a real difficulty, one that it must be feared will for long beset the serious study of evolution. In order even to choose subjects for his inquiry, still more in order to pursue them, the student of evolution needs the peculiar knowledge and experience—the whole apparatus, in fact,—which only the collector possesses. In all this he is too often deficient. It is much if the very names of common objects of natural history are familiar to him, and the world of "good species" and "bad species" is unknown. Seldom even can the two classes of men greatly help each other. The collector finds the evolutionist ignorant of what he regards as the rudiments; he only vaguely perceives the other's purposes and is not greatly interested in them. His collection was made with different objects altogether, and though with the best will in the world he puts it at the service of any one who will work at it, he cannot make it serve a purpose for which it was not designed. The other leaves disappointed. His questions are mostly unanswered, and he is tempted to feel that the methods of the collector are narrow and that he has missed his opportunities. Now both men are right. The future is with the evolutionist, but it is the collector who has made it possible for him to begin his work.

The time has surely come for some attempt at closer collaboration between these two classes of students. Collecting in the ordinary sense, amassing once more the specimens of each species and variety in thoroughly familiar ground, such as England and Central Europe, is a well-

worn pursuit from which little novelty can be hoped. Such collecting is little better than a pastime, a thing of momentary value. On the other hand, by only a slight modification in his practice the collector could make his collection a body of evidence of high scientific value, evidence which if we are to progress with the problem of species has become a necessity. Not the less will he find in his work the peculiar fascination which collecting exercises over so many minds.

What the evolutionist asks of the collector is simple. It is, in brief, that he will give to the collection and observation of a few—and for choice, variable—species, the time and effort that he now devotes to the bringing together of rarities. This is a very simple thing, but I believe that at this moment it is a work which scarcely any one has in hand. Scattered over the world there are hundreds of people employing their energies and exercising a high degree of skill in the collection of natural objects and in breeding specimens for their cabinets. If only a few of these had set to work in the way required, we should perhaps already know a good deal about the origin of species.

The fancy is common that in order to investigate evolution, experiments of an elaborate character, dealing with the causes of variation and other recondite subjects, are essential. This is a complete mistake. The first requirement is to know the plain facts as to the occurrence of species and their varieties in the state of nature. To pursue the question of the interrelations of these forms and the degree to which characters are inherited, artificial breeding must of course be undertaken; but even such breeding involves in some cases little more than what many of the best collectors —Lepidopterists at all events—are already undertaking for the sake of rearing perfect specimens.

It may be remarked that variation is conspicuously a subject which can be studied by simple means. Since, moreover, it is essential that each observation should be made repeatedly by different observers in different localities, for a long time no one need fear that he is uselessly

traversing exhausted ground. We are still at the very beginning.

In what follows I purpose to take a few examples, most of them easily accessible, in illustration of the work in view, hoping that those who propose to devote themselves to such questions may be thereby assisted in the choice of a subject, and that these illustrations may afford some indication of the methods which are required. While specialists will already be familiar with most of the subjects chosen, to students of evolution there will be nothing novel in the statement of method.

Variations so definite as to suggest that they may possess something of specific distinctness may roughly be divided into two classes. In the one the variety is found associated with the type, mingling and, we must suppose, breeding freely with it; in the other the variety is found partly or entirely separate from the type. It is perhaps especially in dealing with cases belonging to the second category that we stand in the greatest need of the co-operation of the general collector; but in order to explain more clearly the particular treatment which is needed in cases of the second class, two examples of the first and simpler kind may be conveniently spoken of.

The first is an example of variation familiar to all entomologists, that of *Amphidasys betularia L.* and its variety *doubledayaria*, Millière. The typical form is the well-known Peppered Moth, a large insect having the whole upper surface of the wings of a white ground colour, almost uniformly speckled with black. The sexes are similar. Of this insect a unicolorous *black* form, the variety *doubledayaria*, is now commonly distributed over the northern and midland areas of England. The special interest of this case lies in the fact that it is one of the few examples in which a variety is known to have come into prominence within recent times. Though the case is notorious in entomological circles its importance has scarcely been recognised by evolutionists. A short summary of the evidence is given by Barrett (1), see also Chappell (3). Briefly the

facts are that this striking black variety, of which the early entomologists say nothing, was first met with as a rarity in the North of England towards the end of the forties. It was figured and named by Millière (6) from an English specimen. The variety lives on the various food plants of the type. In 1865 it is reported that most of the males attracted by females exposed at Manchester were of this dark form. In a few years it had spread through a great part of the Midlands. In 1870 the black form was almost as frequent as the type at Newport, Monmouthshire, and a few years later the typical form had almost entirely vanished from that district. At Chester none but black specimens have been met with for many years. In recent years casual specimens have been taken in various English localities as far south as Berkshire. On the continent the appearance of the black variety was much more recent. A dark specimen—though not so dark as *doubledayaria*—was recorded in Belgium in 1886 (4), and again intermediates are reported from Belgium in 1894 (5). In the past ten years the black form has spread over a great part of Germany, as far as Silesia and Dresden (8) (p. 316).

Here then is an actual case of the establishment of a natural variety. If only the Lancashire entomologists had put by 100, or even twenty, specimens of *betularia* taken at random year by year, what a splendid record it might have been! But though there is no such complete evidence as this, the leading fact is clear. In a short time, some fifty years, a dark form has supervened on the light one. Now did this come about by the gradual shifting of the average form through all stages of grey to black? It is practically certain that it did not. The change came about by the success of a particular dark strain or strains. Not that it must be supposed that intermediates are unknown. On the contrary, Mr Barrett, who kindly showed me several of them, tells me that he believes that they were formerly more common than they are now. Moreover, in Belgium it seems clear that an intermediate strain has established itself, and intermediates are also said to be plentiful in the Rhenish Provinces and Westphalia, together

with totally black forms. In some parts of this district, however, as in Barmen and Crefeld, the typical *doubledayaria* is said to be already the commonest form (8). But there is no doubt both that the totally black form existed at an early stage in the transformation side by side with the light one, and that in localities where both occur the same is still the case. The course has not been that the insects of each successive district have become more and more tinged with black, till they culminated in *doubledayaria*, but rather that *doubledayaria*, or less often one of the intermediate forms, spread into, or at least appeared in the area, and either co-exists with the type or has replaced it.

There is great need for experimental and quantitative evidence as to the inheritance in crossed and thoroughbred strains, and the subject is probably well suited to such work. Nevertheless the results of the few experiments that have been made are in accordance with the expectation based on the simultaneous occurrence of the forms in nature, that there is imperfect blending between type and variety, for Steinert (9) raised from a typical black wild female found at Dresden a brood consisting of seventy-five *betularia* and ninety *doubledayaria*. Two of the *betularia* were slightly darker than the normal form.

Similar changes are said to be taking place in several other British species of Geometræ, but the evidence seems to be less clear and less extensive than it is in the case of *betularia*.

Here, then, we have the means not merely of studying the relations of variety to type, but also of actually witnessing a case of evolutive change. If some one will make it his business to collect samples of *betularia* from various districts he will certainly make a valuable contribution to the science of evolution. All that is needed is that wild samples should be taken at random, fairly illustrative of the population of each district, and sorted into classes, (1) normal *betularia*, (2) light intermediates, (3) intermediates, (4) dark intermediates, (5) typical *doubledayaria*. A simple way by which something might be done would be to arrange year by year for the exposure of a few fresh females reared in

captivity, when the attracted males could be taken in the way often followed with such species. This would of course only give information as to the male population, but that would go far towards answering the question.

The numbers which it would be desirable to collect in each sample should of course be as large as possible, but there is a tendency among those not practically acquainted with such problems to greatly exaggerate the need for excessive numbers. To rightly assess the importance of minute and insensible variations, doubtless vast numbers of individuals are needed, but in cases like that of *betularia* probably a very moderate number would in the first instance suffice to give a broad but reliable indication of the facts. If in 100 specimens[1] there were thirty dark and ten intermediate, the subsequent collection of 1000 would probably change the absolute percentages a little, but it is unlikely that it would materially modify any conclusion based on the first sample.

No doubt in districts where the variety was very scarce large numbers would be needed, and it is doubtful whether such cases are suitable for statistical investigation. In the Cambridge district, for instance, *doubledayaria* is still almost unknown. In addition to a typical specimen of the variety I lately took, I can learn of only two others, which were taken severally some years ago. The ordinary form, however, is common, and it is likely that a few hundred specimens would contain no *doubledayaria* at all.

In order to follow up the statistical study of wild specimens and to obtain experimental proof of the modes of transmission of the varying characters by artificial breeding, a wider range of observation is necessary, and, from a few experiments only, contradictory results are almost sure to follow.

The difficulty arises especially from the fact that the power of transmission of the individual is undoubtedly affected by its own back-breeding, a quantity for the most part unknown. In illustration of this I may refer to the discrepancy between the results obtained by Burckhardt,

[1] It is assumed, of course, that the specimens are collected in such a way as to preclude the possibility that they are all one family.

referred to by Standfuss ((8), p. 307), in crossing the varieties of *Spilosoma lubricipeda* and the results obtained by several observers in this country working with the same species. It is well known to entomologists that of this species there is a peculiar dark form, *zatima*. Cram. found in Heligoland.[1]

This variety has been observed to breed true when paired (7). Now there is good evidence that when the Heligoland variety is crossed with the type, the offspring may consist of the type, of true *zatima*, and of certain lighter forms of this variety, named var. *intermedia* of Bang-Haas. But between this latter variety and the type, though crosses of the most various nature were continued for four years and an immense series of specimens reared, the intermediates were always wanting (8). But this is not because such intermediates cannot exist. In England a dark race of *lubricipeda* has been artificially reared by selection (starting from naturally dark specimens) by the Yorkshire entomologists, which approaches rather nearly to the true *zatima*. These artificially produced forms are known as *radiata*. In a series of them belonging to Mr. Porritt's strain, kindly shown me by Mr. Barrett, are many of both sexes which quite fill up the gap between *intermedia* and true *lubricipeda*. Still more completely is this interval filled by a series lent me by Mr. W. H. B. Fletcher, which had been reared by crossing the partially artificial *radiata* with another variety.

Again Standfuss records that by crossing the natural dark variety *eremita*, a form of *Psilura monacha*, with the type, a brood was produced containing one female *eremita*, the rest being all typical *monacha*. This cross-bred female he again crossed with a male of the type and there resulted twenty-two normal *monacha*, twenty-three typical *eremita*, and six individuals in which the characters of type and variety were *asymmetrically* mixed. Such a brood is a clear proof of imperfect blending of the two characters.

[1] Whether it is there associated with the type or not I cannot find clearly stated by any authority, but I gather that the normal as well as the variety occurs in the island.

But Mr. Fletcher by starting with a normal female *monacha* raised by selection among her offspring, successively repeated for five years, a series of which a few in the latter years are practically *monacha*, and in this series numerous intermediates occur.

In each case the indication is the same, that the results depend on the strain employed. It is greatly to be hoped that these experiments may be repeated and detailed statistics of the results published.

So far as I can learn those that have been hitherto performed have not been for the most part recorded in any detail and the specimens have been largely dispersed.

The other case which I shall take in illustration of a similar phenomenon is that of the hairy and smooth forms of the Cruciferous plant, *Biscutella lævigata*. I have had opportunities of observing this plant in several localities in the wild state, and in a recent communication to the Royal Society Miss E. R. Saunders has given the results of experimental cultivation and artificial fertilisation of its varieties. The facts are these. The plant is a common Alpine species with a rather wide distribution. Some authorities describe it as hairy, others as glabrous. Generally it is stated that it may be either hairy or glabrous. The essential fact to the student of variation is that in certain localities the hairy and the glabrous forms may occur side by side in the same meadow or on the same rock. In my experience the hairy form is the commoner. The plant is common, for instance, in the Val d'Anniviers, at Bérisal, near Airolo, and elsewhere, exclusively in the hairy form. The leaves and stems are thickly covered with a short pubescence, varying in texture, but commonly the hairs are stiff. But in the Val Formazza, side by side with the hairy plants, are others exactly resembling them in the rest of their structure, but having the surfaces of the leaves and stems smooth and devoid of hairs. In the great majority of these smooth-leaved plants, however, the margins of the leaves and the leaf-teeth, often also the mid-rib, bear hairs: it is the *surfaces* which are smooth. In this valley, both

above and below the Tosa Falls, the plant abounds in both these two forms, the smooth being on the whole the more abundant. Now between these two there are various intermediate forms making a complete series connecting them. But though the degree of hairiness varies, and the degree of smoothness varies, the immense majority of plants in the locality can be referred unhesitatingly either to the hairy class or to the smooth. Plants which are so intermediate in character as to cause the observer any doubt as to the class to which they belong are decidedly scarce. Owing to the fact that the plants increase much by runners, forming new crowns which afterwards separate, it is impossible to say which should be counted as "individuals," and hence it is not easy to see how a reliable numerical measure of the discontinuity can be obtained, but that the fact is substantially as stated there is no doubt.

The question then arises, how is it that these two distinct varieties can exist side by side, exposed to the visits of insects, without regressing to the mean form, which, as was stated, is comparatively scarce? To determine this Miss Saunders made artificial crossings of the two varieties in the Cambridge Botanic Gardens. The mongrel seedlings, judged by their first few leaves, consisted of some hairy, some smooth, and a large number of various intermediate forms. But it was found that as these seedlings grew older, while the hairy continued to put up leaves which were hairy, the intermediates in their later leaves turned to smooths. The original smooths also to some extent became smoother. When, therefore, the well-grown mongrel plants are examined they present just the same appearance of discontinuity which the wild plants at the Tosa Falls do. This discontinuity is, therefore, the outward sign of the fact that in heredity the two characters of smoothness and hairiness do not completely blend, and the offspring do not regress to one mean form, but to two distinct forms. The variety, in short, is not "swamped by intercrossing".

Which may be the older form we will not venture to discuss. It is, however, of no small interest to note that Miss Saunders found that the smooth plants suffered far

more from slugs—which attack the species greedily—than the hairy ones did. We have no information on the point, but it is just possible that some creature which elsewhere eats off all the smooth seedlings is absent from the Val Formazza. The eggs of *Pieris napi* are laid on both kinds, and the larvæ can eat both kinds, but whether they prefer either I cannot say.

These two cases must suffice as examples of those varieties which, coexisting with the type, have the quality of distinctness. Many such cases will be known to those who collect animals or plants. Of few indeed have any satisfactory accounts, much less statistical records, been prepared showing how or in what proportions the various forms occur in nature; and in still fewer has any cross-breeding been systematically carried out. In most cases all that we know is the bare fact that such a species has such a variety commonly or occasionally found associated with it, if so much. Rarely do we know for certain whether intermediate forms exist or not. On such points the writings of systematists are too often consulted in vain.

In all these cases the course to be followed in the first instance is simple, and consists in examining and, if possible, preserving a fairly numerous sample taken at random from as many distinct localities as possible. In recording the results of such collecting, the essential points to be noted are these:—

(1) That the variety occurs associated with the type; that it is scarce or abundant.

(2) Whether the variety is connected with the type by a series of intermediate forms or not.

(3) If intermediates do occur, it is then to be determined whether in each sample the specimens can for the most part be readily sorted into type and variety, or whether specimens that are so intermediate as not to be thus referable are common.

(4) The above questions to be answered separately for each sample from a distinct locality.

With regard to the third point, it is obvious that when-

ever it is possible the numerical proportions which the type, the variety, and the intermediates bear to each other should be determined carefully by statistical seriation. This must be done in suitable cases by measurement, and in other cases by the constitution of a series of grades and counting the specimens belonging to each grade. But even where by the intrinsic circumstances of the case or by the conditions under which the collecting or observations have to be conducted, numerical estimations cannot be made, it is certain that most valuable material would be provided merely by answers to the first two questions, with a careful statement of impression as to the third point. These answers would not merely show roughly whether the case was likely to repay further and special investigation, but they would provide that general outline of the properties of species in regard to variation which all the labours of the systematists have failed to provide, though it has become an urgent necessity to progress. The systematists have given us a catalogue of the material, treating it from the cataloguer's standpoint, but in the light of the study of variation we are still at the first recension.

In a subsequent part the application of these methods to the special problems presented by local races will be considered.

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(To be continued.)

THE APPLICATIONS OF CRYSTALLOGRAPHY TO PHYSICAL CHEMISTRY.

THE chemistry and physics of gaseous substances have been for so many years the subject of such careful study that we are now in possession of complete and highly organised working hypotheses respecting most of the properties of gases. And although it can hardly be said that the facts up to the present elicited from the study of liquid substances have led to the establishment of satisfactory theories concerning the molecular physics of liquids, yet the pioneer work has been so well done that we are within measurable distance from the acquirement of very complete knowledge of the molecular constitutions and molecular tactics of liquids; the methods of attacking the problems involved, which have been based on such properties as the molecular volume, refractive power, capillarity, viscosity, thermal properties, etc., are of so powerful a nature that the situation will probably be to all intents and purposes captured within a very few decades.

Whilst, however, the great measure of success which has attended scientific inquiries into the molecular properties of liquids and gases must stand as an abiding monument to the industry of the nineteenth century, our comparative ignorance of the chemistry of the solid state will, in years to come, testify to that high order of unassailability peculiar to the questions at issue which seems to have almost nullified all attempts to learn something of this department of chemistry. That we know multitudes of facts respecting solids cannot be denied, but it is scarcely an exaggeration to say that the framework of chemical theory surrounding those facts is of too flimsy a nature to hold them together, and the absolute value of the theories as affording indications of the lines upon which future work should be conducted is extremely small. We learn little of scientific importance from most of the physical or chemical facts at present known about solids; such a fact as that the great tough-

ness of pure copper disappears in presence of a trace of alloy, although of much practical importance to coppersmiths and to engineers is scientifically barren, because it has not yet tended towards the attainment of the only aim of all scientific work, namely, the acquirement, at some infinitely distant date, of a perfect knowledge of the constitution of matter.

The history of the physico-chemical investigation of gases, liquids and solids is of the very highest interest, not only as teaching the classic methods which have done such service in the past, but also as indicating the lines upon which work will probably be conducted in the future. Comparatively early in the century the physical and chemical examination was prosecuted with an energy which rapidly furnished us with a fairly complete theoretical knowledge of gases; and it was not until much later that a vigorous attack, directed against the more complex problems presented by liquids and liquid solutions, led to a vast increase in our store of knowledge of matter. It cannot be doubted that in the future the investigation of the physico-chemical properties of solids will be undertaken by chemists with a unity of purpose which will speedily bring to comparitive maturity our present embryonic knowledge respecting solids. Under these circumstances it may not be unprofitable to consider some few of the peculiarities which stamp solids as a class apart from liquids or gases, and to point out in what way these peculiarities affect the questions at issue and how they may easily lead astray.

Many of the laws governing gases are immediately applicable to liquids, the differences between the two states being largely differences of degree and not of kind; the truth of this statement has been repeatedly exemplified by the masterly work of van der Waals on the continuity of the liquid and gaseous states and by many other investigations published during recent years. The brilliance of the results which have been obtained by drawing analogies between liquids and gases would at first sight seem to stamp as logical those attempts to learn something of the physical chemistry of solids which have been based on analogy; but

as indicated above little absolute difference of kind exists between liquids and gases, and it is consequently advisable, before attempting to apply laws governing these two states to solid substances, to inquire whether the differences between solids and liquids are also, in the main, differences of degree rather than of kind, and, if considerable differences of kind do exist, to ascertain whether they are of a nature likely to vitiate arguments based on analogy.

At the very outset of the inquiry we are met by a well-marked difference of kind between solids and liquids. The particles composing a liquid are free to move, and under ordinary conditions bear no fixed and definite orientation one to the other; although Perkin's work on magnetic rotation probably indicates that under certain conditions, as when placed in a magnetic field, the particles of a liquid do arrange themselves in some regular manner. Almost all solid substances, however, are crystalline and an amorphous or non-crystalline solid is a substance of some rarity, most substances which are casually described as amorphous being in reality crystalline. That the arrangement of the particles of a crystalline body is not a structureless or amorphous one is evident from a very superficial examination of such properties of crystals as the colours, solubilities, electrical properties, cleavages and refractive indices; these properties being different in different directions in the crystal demonstrate conclusively that the arrangement of the parts or structural units of the crystal varies with the direction. And we are now, thanks to the labours of Sohncke, Barlow and others (compare Miers, SCIENCE PROGRESS, 1894, i., 483; 1896, iii., 129), acquainted with all the possible kinds of arrangement of these units of the crystalline structure. It is important to consider in what way this homogeneous arrangement existing in crystalline substances but not in liquids or gases can affect analogies drawn between liquids and gases on the one hand and crystalline bodies on the other; the best method of showing the kind of disturbance introduced and the ways by which it may be eliminated is probably to consider a few actual cases.

The careful study of the refractive indices of liquids and

gases has shown that a refraction constant of an additive nature—termed the molecular refraction—can be calculated; if W be the molecular weight of the compound, d its density and n its refractive index for a particular wave-length of light, the molecular refraction R is calculated from the equation :—

$$R = \frac{W}{d}(n - 1).$$

On determining the molecular refractions for a large number of organic liquids, Gladstone found them to be the sums of so-called atomic refractions of the atoms composing the molecule of weight W; these atomic refractions are themselves calculated from the observed molecular refractions of many liquid organic compounds. It is found further that the atomic refraction of a carbon atom attached to four different atoms or groups of atoms is different from the atomic refraction of a carbon atom bound to only three atoms or groups of atoms; the same is true of other atoms bound to different numbers of other atomic groups. A knowledge of the molecular refraction is frequently of great value to the organic chemist, as for instance in determining whether any particular organic compound is saturated or not, namely, whether any of its carbon atoms are attached to less than four other atoms or groups of atoms, because the molecular refraction of a saturated compound differs from that of an unsaturated compound of the same molecular composition. On attempting to apply the idea of molecular refraction to crystalline substances, difficulties arise, because in the cases of most such bodies the refractive index varies with the direction; it is thus not at first sight clear how the varying refractive index of a crystalline substance can be dealt with so as to yield a single molecular refraction constant from which results analogous to those obtained in the case of liquids might be derived. It has, however, been shown that from the several refractive indices of a crystalline solid a certain mean refractive index can be easily calculated which would be the refractive index of the substance if the regular arrangement of its particles were destroyed whilst its density were maintained constant; then

by using this mean refractive index to calculate a molecular refraction by the formula quoted above, it is demonstrated by taking many examples that the molecular refractions of crystalline salts are, like those of organic liquids, additive quantities within the limits of experimental error, that is to say, the molecular refractions are the sums of the equivalent refractions of the atoms or groups of atoms (radicles) composing the molecules.

Evidently, in the foregoing case, the differentiated structure of crystalline substances prevents the immediate application to them of a law governing liquids and gases although the hindrance so introduced can be put aside by a comparatively simple operation. In the case of other properties of crystalline substances, however, so simple a method of eliminating the complication introduced by the structure will scarcely avail, although the method adopted above for maintaining the analogy by a process of averaging will doubtless be ultimately found applicable to a large number of properties.

Thus the famous law of Dulong and Petit which states that the product of the specific heat and atomic weight of an element is a constant, is only a very rough approximation to truth. This is doubtless due to the fact that the specific heat is not wholly dependent on the nature of the element but partially also upon the crystalline form of the element; we know that two different crystalline forms of the same chemical substance possess different specific heats as in the case of diamond and graphite or of crystalline and amorphous selenium. This quite apart from the apparent necessity of obtaining data, not at the same temperature but at equivalent temperatures, namely, at equal distances from the critical temperatures of the substances concerned, is a sufficient reason for the failure to so modify the law as to make it exact; since the specific heats are measured on elements the crystalline forms of which are totally dissimilar, the influence of the crystalline structure on the specific heat is wholly irregular and consequently no simple law could govern the data with rigid accuracy. Only after the specific heats of the elements have been calculated from the specific

heats of the liquid substances or their compounds under comparable or equivalent conditions apparently will it be possible to re-cast the original law into a form in which it will hold with accuracy. Naturally, the same kind of criticism is applicable to many other properties, such, for example, as the electrical conductivity; since all the solid metals and most other substances we have to deal with are crystalline, composed of minute crystals of which the conductivity varies with the direction and of which the average conductivity is presumably dependent on the crystalline form, no general quantitative law connecting the electrical conductivity and the chemical nature of the material can be formulated until the influence of the crystalline form of the tiny individual crystals can be expressed in figures and eliminated.

Many other instances might be quoted in which crystallographic causes operate to render obscure the laws governing various classes of chemical phenomena; an instructive case of the kind arises in connection with the heat of formation. During the formation of a chemical compound by the union of certain materials a perfectly definite quantity of heat—the so-called heat of formation—is absorbed or liberated; this heat of formation is of course a measure of the energy-content of the compound. The heats of formation of substances have long been regarded as a highly constitutive property, the continued study of which cannot fail to yield important information respecting the manner in which the atoms constituting a chemical molecule are linked together; large numbers of organic compounds,—gaseous, liquid and solid—have therefore been examined in order to ascertain the exact nature of the relation holding between chemical constitution and heat of formation. The results up to the present obtained have hardly been commensurate with the work done on the subject owing to the many inherent difficulties which have been encountered. It is, however, somewhat remarkable that the influence which the crystalline form of a solid exerts on its heat of formation has to a large extent escaped consideration so that we find, for instance, the heat of formation of solid substances compared with those of

other and liquid compounds; further, the crystalline characters of the solids examined are scarcely ever specified. The kind of misunderstanding to which this may lead is well exemplified by the following case. The curious statement has been on record for many years that the latent heat of fusion of chloral hydrate prepared immediately before examination by melting and then soldifying is less than that of the same material solidified some days before examination; this mysterious result has a purely crystallographic cause which completely escaped the notice of the original investigators. Crystallographic examination of chloral hydrate shows that when it solidifies from the molten state it crystallises in long thin needles (β) but during the following ten days or so these needles slowly change into stout rhomboidal plates (α) until finally the whole material consists of plates formed at the expense of the original acicular form; the explanation of the variable heat of fusion is now simple. The β modification of chloral hydrate has a low latent heat of fusion, only about one half that of the α form; during the prolonged and gradual conversion of the β into the α modification the difference between the two heats of fusion is as gradually liberated and dissipated, the result being that to convert the α modification into liquid at the same temperature requires the absorption of about twice the quantity of heat necessary to liquefy the β modification under similar conditions.

Examples such as the above, indicating the important influence of the crystalline form in complicating physico-chemical problems relating to solid bodies might be multiplied indefinitely but all point the same moral, namely, that since the crystalline form is practically always a complicating feature, it is an imperative necessity that all exact work on solids, whether chemical or physical, should be accompanied by a careful study of the crystallographic questions involved; until the original investigator, more especially the physicist, recognises and acts upon this conclusion, his work on solids, though doubtless of much value in the workshop, will be comparatively unfruitful of results of scientific interest.

The intimate connection of crystallographic phenomena with many physico-chemical problems relating to solids having been thus pointed out it is of some interest now to consider what views respecting the chemical constitution of crystalline bodies are possible and reasonable, and how these may be expected to affect physical chemistry; the chemical constitutions of crystalline substances have long been a favourite theme for discussion, but owing to the very gradual accumulation of facts the last word on the subject will long remain unsaid.

A considerable proportion of crystalline substances exist in several forms which differ crystallographically amongst themselves and are consequently termed polymorphous modifications of the same compound; thus the α- and β-forms of chloral hydrate are polymorphs. Since polymorphs are in general very readily converted one into the other by change of temperature, and their properties would seem to differ only in physical and not in chemical respects, it is generally considered that the chemical molecules in the various polymorphous modifications of a compound are identical; and inasmuch as the physical properties differ markedly it has, until recently at least, been customary to presume the existence, in polymorphous modifications, of physical molecules composed of aggregates of chemical molecules, the number of the latter constituting these aggregates being different in the various polymorphs. This distinction, being very vague and perhaps somewhat metaphysical in tendency, has like many of the household terms of physics and chemistry, proved invaluable as a disguise to our ignorance; although from its very nature somewhat difficult to attack, the notion of a distinction between physical and chemical molecules has a weak point in that it is at present quite unnecessary. Tutton, from a consideration of the results of much experimental work in the light of Barlow's investigations into crystalline structure, has demonstrated the probability that, at all events in the cases which he has examined, a crystal is built up merely of chemical molecules, and that these are not necessarily aggregated together in any such way as is

suggested by the idea of physical molecules; we may, however, take a step beyond this and point out that so far as our present knowledge goes there is little foundation for the view that amongst polymorphous substances the units of which the crystalline structure is built up are other than simple chemical molecule.

Barlow has shown that by distributing an infinite number of bodies in space in such a way that the arrangement around each is in all respects similar, a number of arrangements are obtained each of which has exactly the symmetry of one or other of the thirty-two crystalline systems; it is obvious that no direct relation exists *à priori* between the symmetry of the system of bodies and that of the component bodies or figures themselves. We must conclude from Barlow's work that a crystal is simply a system built up of particles which do not in general possess the same symmetry as the system itself, and which we may suppose to be the so-called physical molecules. Now in the case of a substance like calcium carbonate which exists in two polymorphous forms, namely as the hexagonal mineral calcite and the orthorhombic aragonite, it seems to demand fewer assumptions to suppose that the physical molecules are the same in the two minerals, the only differences lying in the nature of the mode of distribution of the molecules, than to assume that the units of which the crystalline structure is built up are of different complexity in the two cases. Many substances, like ammonium nitrate, exist in several forms, each of which is stable within certain limits of temperature but changes into another polymorphous modification so soon as those limits are exceeded; in these cases also it is only natural to suppose from our present knowledge that at a certain temperature one crystalline modification exists, the molecules of which are arranged in a certain definite manner, and that as the temperature drops the structure contracts, the molecules shrink nearer together, until ultimately when the temperature of change is reached, the structure jams and the same molecules arrange themselves anew in quite a different manner. When one polymorph changes into another

we do know that the point-system or structure changes, but we do not know that the unit of the structure alters at the same time; and since the change of point-system or of arrangement of the molecules is sufficient to explain all the facts, it is an offence against the law of parsimony to assume that the physical molecule changes during the interconversion of two polymorphous modifications of a substance. It should, perhaps, be pointed out that capillarity determinations have shown that many substances have not the same molecular weight in both the liquid and the gaseous state, the free particles in the liquid being apparently aggregates of gaseous particles; whether these polymolecular aggregates persist in the solid state is not yet known, but it would seem that no complication of this kind should affect salts, these being, according to Crompton, monomolecular in the liquid state.

Although Tutton's purely crystallographic work indicates that in a number of cases the chemical molecule is identical with the physical molecule in the crystalline state yet it is not to be expected that our knowledge of the relative weights of these molecules will be in the main derived from arguments deduced from the measurement of series of crystalline salts; the investigation is of so laborious a character and the interpretation of the results so liable to error as to suggest that the methods of physical chemistry are likely to lead more rapidly and surely to the desired end. Nernst has indeed indicated a method which will probably be found of general applicability, and which depends on a remarkable property of chemically related crystalline substances. Two compounds when very closely related chemically are nearly always obtainable in crystal of almost, though usually not quite, identical form; the structures in which the physical molecules of two such substances are arranged are thus of the same pattern, and the compounds are said to be isomorphous. A striking peculiarity of isomorphous substances lies in their power of crystallising together from solutions containing them both, crystals being deposited which contain the two salts in varying proportion, depending on the composition of the

liquid solution; these mixed crystals are termed "solid solutions" by van't Hoff, partly because their properties are in the main means of the corresponding properties of the two crystalline salts composing them, in the same way that the properties of a solution of one liquid in another is in the main a certain mean of the corresponding properties of the constituent liquids. If two non-miscible liquids lie in contact with each other, and a third substance A soluble in both these liquids be added to the mixture, equilibrium is after a time set up in the following manner; the substance A distributes itself between the two non-miscible solvents in such a way that the ratio of the amounts of A dissolved in equal parts of the two solvents (the so-called partition-coefficient) is independent of the actual concentration, provided only that the concentration is not great, that the temperature remains constant, and that the molecular weight of A is the same in each of the two solvents. Should, however, the molecular weight be twice as great in one solvent as in the other, the partition-coefficient is not constant, but becomes constant if a particular term of the ratio is squared before calculating. This method of determining molecular weights is applicable not only to two liquid solvents in contact but also to a liquid solution in contact with a solid solution of the same substance; thus if a series of aqueous solutions containing the isomorphous salts, potassium chlorate and thallium chlorate in various proportions be caused to crystallise at a fixed temperature, mixed crystals are deposited of such composition that the proportion of potassium chlorate in the mixed crystals divided by the proportion of the same salt in the liquid solution in contact with, and therefore in equilibrium, with them is a constant, and thus independent of the actual proportions of the two salts present in either liquid or solid solution, provided that the potassium chlorate is not present in large proportion. It is hence evident that in the crystalline state potassium chlorate has the same molecular weight as in solution, and that, therefore, the physical molecule is identical with the chemical molecule in the crystalline substance. Several disadvantages embarass the easy application

of the method; it could not be expected to hold rigidly as concentrated solutions such as are frequently employed, and the electrolytic dissociation occuring in aqueous solutions introduces a complication which cannot always be successfully eliminated. There is little doubt that Nernst's work in this field opens the way to a most powerful method of determining the molecular weights of crystalline substances.[1]

These and many similar considerations seem to indicate that at present the only legitimate view concerning the matter is that the molecular weight is in general the same in both the liquid and the solid state; there is thus a complete analogy not only between the molecular properties of gases and liquids on the one hand but also between those of liquids and solids on the other. But just as in cases of the former kind exceptions arise, the molecular weight not being the same in the gaseous as in the liquid state, so doubtless exceptional cases will be found to exist amongst the latter. Consider such cases of polymorphism as that of the diamond and graphite or of the red and yellow crystalline modifications of phosphorus; these are frequently quoted cases of allotropy of which the generally recognised meaning is that it is merely polymorphism occurring amongst the elements. The differences between the diamond and graphite or between red and yellow phosphorus are far greater than those observed between ordinary polymorphs such as those quoted above; it would therefore seem only reasonable to accept the view that such substances are not ordinary polymorphs but chemical polymerides just as oxygen O_2 and ozone O_3, or acetylene, C_2H_2, and benzene C_6H_6, are polymeric substances. Again, it is known that the molecular weight of sulphur only corresponds to the formula S_2 at temperatures high above its boiling point whilst near the boiling point the gaseous molecules are at least three times as large and

[1] Whilst this article was passing through the press, Dr. Andreas Fock published a valuable paper in which he shows, by applying the equilibrium method briefly described above to many pairs of isomorphous salts, that all the salts examined have the same molecular weight in the crystalline, as in the dissolved condition.

therefore probably have the composition S_6. Further, when sulphur is heated from its melting to its boiling point it undergoes a remarkable series of changes; near the melting point sulphur is a fairly limpid straw-coloured liquid which does not change rapidly in colour or consistency until a temperature not far below the boiling point is reached, when it suddenly assumes a deep-brown colour and becomes so excessively viscous that the containing vessel may be inverted without the liquid running out. It is scarcely conceivable that a change of this kind can be due to anything but a change in molecular complexity; and, since changes in molecular complexity certainly occur in gaseous sulphur and almost as certainly in liquid sulphur, it is very possible that the six or seven different polymorphous crystalline modifications of solid sulphur have not all the same molecular weight but that some are polymerides of others.

The physico-chemical investigation of crystalline substances may be confidently expected to yield brilliant results in that department of chemistry which deals with tautomerism occurring amongst organic compounds; this subject is one which every organic chemist recognises as presenting great difficulties and some of the ablest physical chemists have for long past applied their most ingenious efforts to its study. Many organic compounds are known which do not behave consistently during the ordinary laboratory operations; in some reactions they behave as if they had a certain constitution whilst in others they seem to possess quite a different constitution. Such substances are said to be tautomeric and are supposed to possess each of the two constitutions at different times, the conversion from the one molecular constitution to the other being very readily brought about. In certain cases two tautomeric compounds can be isolated and examined; their investigation in the liquid state is, however, extremely difficult owing to the complication introduced by the tendency to interconversion. Two tautomeric forms, if in the solid state, should be much more easily investigated than if they were in the liquid condition, because in the former case the tendency to rearrangement of the atoms composing the molecule would naturally

be much less and because crystallographic methods could be used to ascertain with ease and rapidity which of the two substances was under examination; up to the present, however, crystallographic methods have not been applied to the study of tautomerism.

Although our knowledge of tautomerism apparently has much to gain from crystallography, yet a protest is necessary against a highly reprehensible practice which has recently been adopted by a certain school of organic chemists. Large numbers of organic compounds exist in several polymorphous forms, and naturally enough there are amongst these substances some, the chemical behaviour of which can be equally well represented by two tautomeric constitutions; in these cases it seems to have become customary to assign one constitution to the one crystalline modification, and a different one to the second modification. This has been done in such cases as that of benzoïn with apparently no regard to anything but the fact that two molecular configurations are possible, and that two polymorphous forms exist; it need hardly be pointed out that such a method of creating new compounds is hardly likely to redound to the credit of organic chemistry.

Should the view advocated above, namely, that the physical molecules or the units of the structure of crystalline substances are in the majority of cases the chemical molecules, we hold the key to many problems which have hitherto successfully resisted all attempts at their solution. The close crystallographic relationships often existing between substances which are not isomorphous, but, nevertheless, are more or less closely related in a chemical sense, have always been amongst the most curious and interesting of crystallographic phenomena; these analogies are termed morphotropic relationships, and have doubtless escaped notice in hundreds of cases owing mainly to the fact that the crystallographic examination of organic compounds has been usually entrusted to mineralogists, who, naturally enough, are not alive to the chemical interests at stake. Here, again, the recital of an actual case will best serve to exemplify what is meant by a morphotropic relationship.

Camphoric acid crystallises from acetone in large transparent tablets containing acetone of crystallisation, and having the composition $C_{10}H_{16}O_4$, $\frac{1}{2}(CH_3)_2CO$, which can be considered as built up on three rectangular axes, the ratios of the lengths of which are $a : b : c = 1{\cdot}2386 : 1 : 1{\cdot}7172$. Camphoric anhydride, which is closely related chemically to camphoric acid, crystallises without acetone in tablets of very similar appearance; these have the composition $C_{10}H_{14}O_3$, and may be referred to the rectangular axes $a : b : c = 1{\cdot}0011 : 1 : 1{\cdot}7270$. The ratio of the axes $c : b$ is almost the same in the two cases, so that many of the angles measured are nearly identical on both kinds of crystals; the crystals of both substances cleave readily along a plane perpendicular to the *c*-axis, and less readily perpendicular to the *a*-axis. In each case the optic axes lie in the plane perpendicular to the *a*- axis, the smaller optic axial angle is bisected by the axis *b* and the double refraction is negative in sign; finally both kinds of crystals are striated in similar ways and almost the same planes are developed on each. The resemblances between the crystals, notwithstanding the great difference in composition, are extraordinarily close and cannot be regarded as due to mere coincidence; they indicate clearly that one plane section of each structure is of the same pattern in the two cases but that there is no such similarity in other sections. Morphotropic relationships of similar natures to the foregoing are frequently discovered; they indicate not only that the kind of grouping of the molecules in the crystalline substances upon which they are observed is of a somewhat similar character but also support the view that the physical molecules in crystalline substances are in general identical with the chemical molecules. The information to be gained by the systematic study of morphotropic relationships is undoubtedly great and cannot fail to well repay investigation.

In the preceding pages an attempt is made to demonstrate that many of those problems of physical chemistry which are now so quickly coming to the fore are so closely interlinked with crystallographic phenomena that to study

the former without a knowledge and appreciation of the latter is to court failure. Further, it may be remarked that the very complications introduced into the molecular physics of solids by the fact of their possessing crystalline structure only strengthens the methods of investigation; the highly differentiated nature of crystallographic phenomena in itself alone renders crystallography one of the most powerful instruments of research at the disposal of modern science.

The chemist has in the past always been curiously apt in seizing upon and applying to his subject any branch of physics which bears in the slightest degree on chemistry; many departments of natural science which our predecessors would have classified as pure physics are to-day so well identified with chemistry that to exclude their consideration from chemical text-books would excite surprise. Has not the time come for the chemist to annex crystallography and to set it in its true position as one of the most promising and fascinating branches of physical chemisty?

William Jackson Pope.

GERMINATION OF SEEDS. I.—THE VITALITY OF DORMANT AND GERMINATING SEEDS.

THE term germination is mostly used to connote resumption of growth by dormant *seeds;* this restriction is neither etymologically nor physiologically defensible, and is now merely a tribute to custom. Germination connotes resumption of growth by any dormant part of a *plant*, for the rationale of this process is in all cases comparable, the particular case of seeds being merely one aspect of its deconnotation. This article deals only with the germination of seeds to avoid undue extension, and primarily with the 'vitality' of dormant seeds, since it is obvious that the former is a function of the latter.

Resistance to time.—Numerous statements have been periodically made about the 'longevity' of seeds; the majority are of little value from lack of detail or of sufficient positive proof. The most notorious are those concerning seeds from sarcophagi of Egyptian mummies. It is now generally acknowledged that no adequate proof of their germination has been produced, the reputed success of some authors having been rather due to duplicity of Arab vendors than genuineness of the seeds. Burgerstein (1) quotes experiments of Unger with indubitably genuine seeds. Corns of *Hordeum hexastichon* from the ruins of Thebes were tested; not one germinated. The same result was obtained with corns of *Triticum* out of tiles made of Nile-mud and straw from the Darhur-pyramid near Cairo. There is nothing extravagant in the idea that mummy-seeds may retain potential life, but there is no proof that this is so; indeed, C. de Candolle (2) states that the wheat was always sterilized apparently before introduction into sarcophagi though no authority is given for this statement.

That the treatment of this subject is not wanting in humour is evident from that which follows: A well was once sunk in the Lias of Shipston-on-Stour; the next year *Glaucium luteum* appeared on the rubbish from the

shaft. No *Glaucium* had previously grown in the neighbourhood. White (3) suggests "that they (the seeds) had possibly remained inert from the time when the deposition of the Lias took place, and upon their exposure to the atmosphere were recalled to life".—This is not only extravagant, but transcends imagination.

An interesting case of reputed 'longevity,' that lacks however positive proof, is one recorded by v. Heldreich in 1873. A heap of mine-refuse on Mt. Laurion in Attica, 3 m. deep, had been removed shortly before he visited the spot. On the recently exposed surface a *Glaucium* was found, which he did not hesitate to consider as a new species, *G. Serpieri; Silene Juvenalis* Del., hitherto unknown there, was also found. To account for their appearance, he suggested that they had been dormant under the refuse-heap since it was first placed there, at least 1500 years ago. De Candolle (2) quotes a statement of A. P. de Candolle, who germinated seeds of *Mimosa pudica* after these had been dormant sixty years, and one of Girardin, who succeeded with seeds of *Phaseolus vulgaris* (haricot), that had lain more than 100 years in the herbarium of Tournefort. It is further stated that in 1850 Brown sowed seeds from the collection of Sir Hans Sloane, of which they had formed part longer than 150 years, and caused them to germinate. The latter cases seem quite trustworthy; one might however object that the seeds had possibly been changed unknown to the experimenters.

Experiments of Peter (4) support the view that the sudden appearance of species hitherto unknown, when soil has been disturbed, is due to persistence of seeds of one vegetation dormant in the earth while later vegetations succeed and to their subsequent germination when conditions changed. He tried to find if soil holds seeds, if it be able to preserve their 'vitality,' as well as which species have seeds capable of remaining thus unputrefied. Soils were tested on the surfaces of which vegetation had not existed for a long time; present covering with wood was disregarded, since tree-seedlings admit of easy distinction and could be eliminated. Soils were chosen concerning which it was

accurately known, whether considerable alteration in constitution of the vegetation had ever occurred. Samples of earth were so taken as to diminish as far as possible the chance of introduction of seeds by various agents. For collecting earth spots in dense forests devoid of vegetation were chosen, chiefly in such as were known to have formerly been fields or meadows. For comparison samples were taken from primæval forests. Layers 8cm. deep were removed; this was done twice, and sometimes thrice, at the same spot. The cultures agreed well. In every test of soil formerly field-soil, the majority of seeds that germinated, at times all, were those of field-plants. The results with earth from forests previously meadows, or perennially forests, were analogous. The woods, primæval ones naturally excepted, had been planted twenty to forty-six years back; Peter concludes hence that the seeds had remained dormant, retaining 'vitality,' throughout these periods. This conclusion is as reliable as any deducible from experiments of this type, which are permeated with uncertainty.

Loss of 'vitality' with age has been recently studied by Burgerstein (1) in seeds of *Triticum* (*vulgare ?*), *Secale cereale*, *Hordeum* (*distichum ?*) and *Avena* (*sativa ?*), aged one to ten years. *Hordeum* preserved 'vitality' best, *Avena*, *Triticum* and *Secale* following in order. Of seeds aged ten years, 70 to 90 per cent. germinated in the case of *Triticum*, *Avena* and *Hordeum*, 1 to 2 per cent in that of *Secale*.

Concerning the longevity of *Triticum*, Acton[1] mentioned that no seed of a sample aged thirty years germinated. This trial was hardly fair, as the mode of storage was not favourable to preservation of 'vitality'.

Naudin[2] adduced this year an old experiment of his; in 1872 he received a bag of dusty earth from the Sahara, from which he reared a *Helianthemum*. In 1895 he received seeds from Gabon enclosed in earth that weighed less than 1kg., but from which he obtained more than

[1] "Changes in the Reserve-material of Wheat on Keeping," Note, *Ann. Bot.*, 7, 1893, 383.

[2] "La longévité des graines," etc., *Bull. d. l. Soc. Nat. d'Acclim. d. France*, Feb., 1897.

twenty seedlings, the seeds of which had been in the earth unknown to the collector. These are the facts which he thinks sufficient to establish 'longevity' of the seed: but they prove little, if anything. No details as to precautions taken against subsequent introduction of seeds are given; even if the seeds were in the original earth, there is nothing to show how long they had been there.

Arthur[1] states that seeds of *Pyrus coronaria* can germinate after dormancy of twenty-three years. The evidence for this is as follows: A barn was built in 1859, the foundation of which was limestone laid on soil. In 1882 the barn was removed, and after twenty days the foundation also. When the bottom stone was raised, two small plants were found near the centre of the stone. He says that there seems no doubt that the seeds had been protected by the building for the twenty-three years, germination having been stimulated after removal of the barn by penetration of warmth and moisture beneath the foundation. No evidence is adduced that the seeds were not introduced by animals or otherwise, for which reason the statement is practically valueless.

Jodin (5) succeeded in germinating peas (*Pisum sativun?*) after dormancy of ten years. Ten were tested; two germinated normally, two abnormally.

Instances of 'longevity' are also given by Meehan.[2] In 1886 he had a few plants of *Lysimachia atropurpurea* in his garden. Next winter this spot was covered to a depth of several feet with earth, and evergreens were planted on it. In 1896 one evergreen was removed and shortly after a plant of *Lysimachia* sprang up at the bottom of the hole thus made. Since none had grown in the garden since 1886, he concluded that the seed had remained dormant ten years. In 1884 plants of *Antirrhinum glandulosum* were raised by him; next year the plot where they had been was covered with earth to a depth of several feet. On turning up the earth in 1894 to a depth of a few feet, one plant came up and flowered. It is very likely that these seeds had re-

[1] *Bot. Gaz.* 7, 1882, 88.

[2] *Proc. Acad. Nat. Sci. Philadelphia*, 1892, 374; 1894, 58.

mained dormant for ten years, but the evidence adduced is too inadequate to enable one to say with absolute certainty that it was so. The seeds might, for instance, have been introduced on a spade, or in many other ways.

The experiments that tend most strongly to prove 'longevity' in certain seeds are those of Girardin and of Brown. The seeds tested had in each case been part of a collection which there was every reason to suppose had not been disturbed, and the age of which was known. But there is no absolute certainty that the seeds had not been interfered with, and this doubt weakens the evidence. There is only one method for settling the question beyond contradiction; large quantities of selected seeds of many species should be collected at some recognised institute after careful previous treatment, and allowed to remain quiescent in a suitable place, to which no one could get access without being known to have done so. These seeds should be periodically tested and the results recorded. The research of Peter, although admirably conducted, only supports strongly the idea of 'longevity' of seeds.

That this 'longevity' is a function varying with the species, and even individual seeds, admits of no doubt. Burgerstein (1) found that deviations due to individual differences in seeds of cereals amounted to from 1 to 16 per cent. Moreover the degree of maturity at harvest-time and the aqueous content at the time of preservation are factors that influence considerably the 'vitality' of seeds.

Resistance to water.—The influence of oceanic agency in distribution of certain plants has been long known; this is, stated in other words, that it has been long known that certain seeds can withstand protracted immersion in salt water. The old experiments of Darwin (6) on this point are interesting. Seeds were placed in bottles with salt water made according to Schweitzer's analysis. The trial was fair, as algæ and marine animals lived long in the water. With Darwin's results are conjointly published some of Berkeley's. The latter tied up seeds in bags and immersed them in sea-water which was daily renewed; immersion lasted three weeks, but since the seeds were

posted damp, and not at once unpacked, total immersion was practically a month. Some of Darwin's bottles were exposed to weekly temperatures of 35° to 57° F., others to daily ones of 46° to 56° F. Seeds were also immersed for six weeks at 32° F., the temperature rising slowly during the next six weeks to 44° F. But seeds thus tested did not seem to withstand the injurious effect of salt water better than those exposed to higher but variable temperatures. The total number of species tested by the authors was eighty-seven; of these twenty-three did not 'survive' immersion of twenty-eight days. *Capsicum* was most resistant, for thirty out of fifty-six seeds germinated after immersion of 137 days, whereas of those of *Apium graveolens* only 6 per cent. germinated after like immersion. Several seeds of *Atriplex* germinated after immersion of 100 days, but those of the dwarf kidney-bean and *Hibiscus Manihot* were 'killed' by immersion of eleven days. All other species tested fell between these two extremes. It is noteworthy that the majority of seeds tried were such as might be expected to resist salt water least, not belonging to littoral plants; indeed, several were garden varieties.

Hemsley[1] cites experiments of Martins and Thuret on this subject. The former tested ninety-eight species; a perforated box was employed with as many compartments as kinds of seeds, being attached to a buoy. After exposure of forty-five days the box was opened; forty-one kinds were rotten; of the apparently sound remainder, thirty-five germinated. Several of the latter were essentially littoral. Fresh samples of the resistant thirty-five kinds were immersed for ninety-three days; only nine withstood the trial. The conditions of these experiments were better than those of Darwin's The experiments of Thuret, in which immersion lasted thirteen months, yielded similar results; seeds of ten species (it is not stated out of how many) germinated, several with more vigour than the controls.

Evidence adduced by Salter (7) tends to prove that seeds can bear prolonged submersion in fresh and salt

[1] *Challenger Report*, 1. 3, appendix 277.

water without injury. In 1843 the channels of Poole Harbour were deepened, the mud raised being deposited on the shore, covering several hundred square yards; a quay was made from the hardened mud: this was never used, and the surface was not disturbed. Early in next spring the surface exhibited abundant vegetation of character totally distinct from that of the flora of the neighbouring shore, containing plants that did not grow within many miles, and one probably foreign to the county. It seemed conclusive to Salter that the seeds must have been in the mud when deposited; he concludes that the seeds fell into two rivers that flow into the harbour, and were ultimately deposited in the channels. The evidence given supports this view strongly; if his hypothesis be accepted, the seeds must have evidently suffered prolonged submersion both in fresh and salt water without losing 'vitality'. But however complete this kind of evidence may be, it is never conclusive, and, as with all induction, can only approximate to truth on reiterated confirmation.

Salter's views are, however, supported by observations of White (3). Mud was dredged from the Avon-bed and the floor of the basins at Bristol by local officials; this was emptied into a disused quarry, the work being done by manual and mechanical agency only. About half an acre was thus covered to a depth of from ten to twenty feet. In next spring a profuse and varied vegetation appeared; many of the species were native on the banks of the Avon above and below Bristol, but had not been found previously at or near this spot. There were also species recorded to grow in the Avon valley, but which had become extinct or extremely rare, as also others not noted before in the district, and some from all quarters of the globe. After comparing all the evidence at his disposal, White concluded that the seeds had been in the mud. This view is as justifiable as that of Salter's, and is open to the same objections. Some seeds may have lain long in the mud, others but a short time; there is no means of settling in any case the duration of submersion, and a safe criterion of judgment as to their origin does not and, moreover, from the nature of the case, cannot exist.

Guppy (8), after study of the agency of the Thames and Lea in the dispersal of plants, adopts similar views. It is said that, speaking generally, all seeds and fruits in the drift of spring have floated through winter; this statement is not merely based on their occurrence in the spring-drift, but also on a series of control experiments. Germination may be delayed till the next year, seeds of *Iris pseudacorus* having germinated afloat after immersion of fourteen months; growth in spring had been stopped by placing the seeds in a cool place. There are scarcely any seeds and fruits common in the drift of these rivers that will not float for months in sea-water and germinate afterwards. Fruits of *Sparganium ramosum* germinated after flotation of twelve months in sea-water. Those of several species 'survived' immersion of three to five months, their limit of buoyancy not having been then reached. Fruits of three species germinated after immersion of months in sea-water, the density of which had been raised to 1·05 by addition of salt; the average density at 0° C is 1·02975.[1] Some seeds, as those of *Lemna minor*, only resist flotation of a few days. It is further stated, and this is important with regard to the observations of Salter and White, that sinking of seeds need not involve loss of 'vitality'.

Ewart (9) mentions that immersion of seeds of peas (*Pisum sativum ?*) for five days in water reduces the percentage of germinating seeds to 60 per cent.; this fell to 0 per cent. after immersion of fourteen days. With *Phaseolus vulgaris* (haricot) the latter percentage is reached after ten days, with *Triticum* (*vulgare ?*), *Hordeum* (*distichum ?*) and *Linum usitatissimum*, after three weeks, whereas 17 per cent. of seeds of *Cannabis sativa* germinated after exposure of four weeks. These experiments contravene distinctly those of Guppy; this is perhaps explicable on the assumption that in this case not dormant but germinating seeds were killed: at all events this is supported by the conditions and results of the experiment.

Horticulturists have long known that steeping seeds in hot water accelerates and augments their power of germina-

[1] *Treatise on Chemistry*, Roscoe and Schorlemmer, i., 266.

tion. Wernicke[1] has examined the action of hot water on various seeds that germinate slowly owing to thickness or impenetrability of their integuments. Seeds of *Acacia molissima* and *Lathyrus sylvestris* were kept in hot water and then allowed to germinate. Of the controls 50 per cent. germinated. The following table gives the results :—

Period of Immersion.	Temperature.	Percentage.
6 hours.	50°C.	60 per cent.
3 „	80°C.	72 „
1 „	95° – 100°C.	92 „

Observations of considerable interest on the action of water, and of air saturated with water, at different temperatures occur in a paper by Just (10); limitation of space precludes more than indication.

Resistance to thermometric extremes and desiccation.— But little work has been done on the influence of intense cold on the 'vitality' of seeds. The earliest seems to have been that of Edwards and Colin quoted by Wartmann (11); seeds of cereals were subjected to a temperature of – 40°C. for some minutes, produced through evaporation of sulphur-dioxide in vacuo. Germination occurred as in the controls. There is a serious objection to this experiment, namely, that the duration of exposure was insufficient to allow the seeds to reach the temperature quoted. Wartmann (11) tested nine species; the seeds were divided into three lots; one served as a control, the second was placed in a thin glass tube, that was hermetically sealed and refrigerated for half an hour at – 57°C. in a bath of suphur dioxide: the tube was finally plunged into a mixture of snow and salt, where it remained fifteen days exposed to the winter cold. The third lot was also enclosed in a glass-tube and placed at first inside a metallic receiver, in which was liquid carbon dioxide, that on solidification produced a temperature of – 78°C. The tube was then covered with paste of solid carbon-dioxide and sulphuric ether and placed

[1] Abst. *Ann. Agron.*, 21, 1895, 544.

in vacuo; the exposure lasted more than twenty minutes, and the temperature sank to − 110°C. Every seed germinated. It might be reasonably urged that exposure was here also inadequate for the acquisition of this low temperature by the seeds. A further communication appeared later (12); seeds of *Æesculus Hippocastanum* were wrapped in the tin-foil and placed in a paste like that mentioned above, in vacuo; exposure lasted two hours and the temperature was at least − 110°C. Germination occurred as in controls. This experiment yields doubtful results; the seeds may, or may not, have acquired the temperature in question throughout. De Candolle and Pictet (13) utilised liquid sulphur-dioxide and nitrogen-monoxide as refrigerating agents. In their first experiment four tubes of thin glass, each enclosing seeds of the same species with metallic fragments, were hermetically sealed and placed in a test-tube, that was also hermetically sealed. The latter was covered with tow, which was constantly sprinkled with sulphur-dioxide, and exposed to air. The tube was thus submitted to gradual lowering of temperature from − 39°C. to − 50°C. for six consecutive hours. All seeds of three species germinated as in the controls. Of twenty-four seeds of *Triticum* (*vulgare ?*), only seven germinated, but the authors attribute this to an accident. In their second experiment thirty species were tested. When − 40°C. had been reached, nitrogen-monoxide was poured several times into the tube with the seed-tubes. At the close of the experiment that lasted two hours the temperature was − 80°C., which was supposed to have obtained for at least an hour. No precautions were taken to graduate thawing, and the seeds were not previously desiccated. Every lot, among which was *Triticum*, germinated except three, in which cases the controls also refused to germinate. The first experiment is probably reliable; the second is dubious on account of shortness of exposure. In a subsequent paper (14) seeds of several species were kept for four consecutive days at a temperature lower than − 100°C., obtained through evaporation of solid carbon-dioxide. Germination was normal in four lots; in four others it did not occur, as also in the

controls. This experiment is important being the first wholly reliable one; there can be no reasonable doubt but that the seeds acquired the temperature throughout. More recently Pictet (15) says that he subjected seeds to − 200°C. in liquid air, and that development succeeded as in the controls. This statement loses much importance through absence of detail, not even the duration of exposure being given. These experiments are in very important relation to the question of the 'vitality' of seeds.

Yet more recently De Candolle (2) gives results of an exposure of 118 days at an average but intermittent temperature of − 41·93° C. The species tested were *Triticum* (*vulgare ?*), *Avena* (*sativa ?*), *Fœniculum officinale*, *Mimosa pudica* and *Lobelia Erinus*. Those of each species were enclosed in capsules of tin-foil which were placed in a sheet-iron box, which was completely filled; the box was soldered and enclosed in a wooden case, that covered all surfaces but one. This was done to preclude injury, for cooling occurred in the refrigerator of a firm of importers of frozen goods. The covering was proved to exert no serious influence on cooling. The cooling agent was cold air. During the experiment refrigeration lasted eight to twenty hours *per diem*; the minimal temperature recorded was − 53·89° C., the maximal − 37·78° C. After cooling the temperature assumed that of the recipient; it is doubtful if this ever reached 0°C. Almost all seeds of *Triticum*, *Avena* and *Fœniculum* germinated normally, but only thirteen out of sixty seeds of *Mimosa*. Out of a large number of *Lobelia* seeds only ten germinated. With *Mimosa* inhibition was not in all cases due to cold, since many seeds in the controls did not germinate; this could not however be said of *Lobelia*. This experiment is important on account of its long duration, no similar one having ever before been made.

Hart[1] mentions that seeds planted under shelter of glass ashore in Discovery Bay germinated and grew; when the sun was at greatest altitude the temperature within the glass was raised one or two degrees, but that

[1] *Jour. Bot.*, 1880, 74.

inside and outside was mostly uniform and constantly as low as 33° F. (presumably – 33°). The excessive cold of winter months, involving upwards of 100° of frost, is also not fatal to mature seeds. Grains of *Triticum* left at Polaris Bay during Hall's expedition, which had lain entirely exposed to the weather for four successive years, germinated in Discovery Bay. Beans (*Faba?*) and peas (*Pisum?*) that germinated had suffered the whole severity of winter on deck with a minimal temperature of – 71° F.

Kerner v. Marilaun[1] cites that the mean temperature of January at Jakutsk and Werchojansk in Siberia is – 42·8° C. to – 49° C., and that – 62·0° C. and – 63·2° C., the lowest temperature ever noted on the earth, have been recorded there. Here also for months the shade-temperature in never above – 30° C.; yet not only do seeds germinate when the conditions become favourable, but plants persist and live.

Guppy (8) shows that enclosure in ice of an English winter does not affect the 'vitality' of seeds enclosed.

Concerning resistance to high temperatures Just[2] states that seeds of *Trifolium pratense* withstand temperatures up to 120° C., if previously desiccated and kept so during the experiment; exposures were from half an hour to two hours. Subjection to high temperatures always involves slower and more imperfect germination than that to lower ones. The more complete the desiccation is, he adds, the less is the injury, but that most careful desiccation will not prevent notable harm with temperatures over 100° C. Later Just (10) experimented partly with carefully desiccated seeds. Dehydration was effected by exposure to sulphuric acid for two days, then in a closed tube to calcium-chloride for ten days at room-temperature; subjection to high temperatures took place in the tube. *Hordeum* and *Avena* were tested. If the tubes were placed in the thermostat at once, the seeds of *Hordeum* were almost killed, those of *Avena* considerably damaged; if, however, they were gradually taken to 100° C., thus effecting a more

[1] *Pflanzenleben*, vol. i., 508.

[2] *Bot. Ztg.*, 1875, 52. Quoted in Just's paper (10) of bibliography.

complete dehydration, the injury was less, and the slower the ascent, the less the harm. At 110° C. the injury was more pronounced; half an hour at 122° C. 'killed' 99 per cent. of corns of *Hordeum*, whereas those of *Avena* withstood exposure of three hours. The fatality was always increased with the duration of exposure. Above 122° C. all seed were 'killed'.

It is curious that resistance to high temperatures is so modified by the aqueous content of seeds, as these and other experiments show, while that to low ones is unaffected, as De Candolle and Pictet have shown.

In Just's paper, results obtained by Haberlandt are quoted which show that desiccated seeds resist exposure of forty-eight hours at 100° C.; these results were confirmed by Krassan.

The results of van Tieghem (16) on this subject contradict those of almost all other observers; it was found that if fruits of cereals were dried at 100° C. till no further loss of weight ensued, 'vitality' was completely destroyed. The contradiction is difficult to explain; that the seeds were not completely dehydrated seems certain, for it is known that certain carbohydrates can be reduced to constant weight at 100° C., and yet yield more water without decomposition at higher temperatures.

It will not perhaps seem out of place to emphasise once more the undoubted fact, that one cannot speak of a constant specific, or even individual, resistance to environment; variation in origin and treatment affects the 'vitality' of seeds correspondingly.

Goff found that seeds of *Triticum* (*vulgare?*) and *Zea Mays* could be germinated six and five times respectively, when the periods of germination were separated by intervals of desiccation for seven days. Van Eyck,[1] who quotes this, obtained like results; seeds that had germinated were removed every twenty-hours and submitted to dehydration in an exsiccator for a week: these operations were repeated till the power was lost. The accompanying table gives his results:—

[1] De la germination répétée des graines, *Ann. Agron.*, 21, 1895, 236.

Species.	Period of Desiccation.	Frequency of Germination.
Triticum (*vulgare?*)	101 days.	14 times.
Zea Mays	35 ,,	6 ,,
Raphanus sativus	28 ,,	5 ,,
Pastinaca oleracea	14 ,,	2 ,,
Daucus Carota	,, ,,	,, ,,
Trifolium sp.	,, ,,	,, ,,
Apium graveolens	7 ,,	1 ,,

Seeds that germinated first always supported interruption best; the effect of the latter was almost always ramification of the radicle and annihilation of its geotropism.

Ewart (9) made somewhat similar experiments with peas (*Pisum ?*), to see the effect of alternate steeping and drying. The seeds were spread on blotting-paper, and left to become air-dried. Only one experiment was apparently made, involving three interruptions. If the integuments remained entire, the effect was more fatal than if visible germination had commenced. Irregular development of radicles was noticed. The 'vitality' of radicles was found greater than that of plumules, and often the cotyledons showed more resistance than both.

That seeds offer considerable resistance to desiccation is obvious from study of the prevalent conditions in torrid districts. Kerner v. Marilaun[1] states that the highest temperature noticed in superficial soil of the earth was at Chinoxo on the Loango Coast near the equator. Here 75°C was frequently exceeded, 80°C was often reached, and once 84·6°C; annuals, however, grew on entrance of the rainy season, the seeds of which had doubtless remained in the soil for months.

Resistance to chemical agents.—A few years ago a research of considerable interest by Cornevin (17) appeared, bearing on the toxic influence of alkaloids on plants. The incentive to the work was given by the theory of dependency of immunity in animals on production of antitoxines. He examined: (i.) the action of the poison of a species on its own seed, (ii.) that of the same poison on seeds of differ-

[1] *Pflanzenleben*, vol. i., 518.

ent species, and (iii.) the physiological reaction between seeds and different poisons. The first case presented two aspects: (i.) that in which the poison exists in the seed and passes into the seedling by a 'kind of heredity,' there being no interruption of the poisonousness of the plant, and (ii.) that where the poison exists neither in seed nor seedling, but is formed later. For the study of the first aspect, *Agrostemma Githago* and *Cytisus laburnum* were chosen, yielding respectively saponine and cytisine. To obviate the objection that the poison did not penetrate, the integument was always removed throughout the research. Neither 'death' of the seeds nor retardation of their germination occurred. For examination of the second aspect, *Nicotiana tabacum* and *Papaver somniferum* were used; the former yields nicotine in the leaves, the latter opium in the capsules. Two experiments on different lines were arranged with nicotine; the first proved that, without actual 'death,' germination was retarded: the second showed that germination cannot occur, or with great difficulty, in a medium impregnated with nicotine. Curiously enough it was found in the case of *Papaver*, that opium excites the power of germination. The isolated action of the best-known opium-alkaloids was then tried; these were morphine, narceïne, codeïne, papaverïne, narcotine and thebaïne, all as hydrochlorates. Narcotine, codeïne and narceïne acted as opium; morphine and thebaïne seemed indifferent, whereas papaverine had an inhibitory action.

In the study of the second of the above-mentioned cases, the action of oleandrine from *Oleander* and andromedotoxine from *Azalea* was tried; these, highly poisonous to man, were here in most cases inoffensive. It is interesting to note that *Phaseolus vulgaris* (haricot) was unaffected by oleandrine, but retarded in germination by andromedotoxine. Colchicine proved harmless in several cases, but affected *Phaseolus vulgaris* seriously; similarly with cytisine. This plant is thus seen to be extremely sensitive to toxic agents, and Cornevin correlates this sensitiveness with that to cold, which precludes its cultivation in northern countries. This sensitiveness is, however,

evinced in many other ways, as Ewart's experiments, and those of others, show.

Examination of the third case mentioned above demonstrated that resistance to these poisons is lessened as duration of exposure to their influence is lengthened.

The question of the effect produced on seeds by deprivation of oxygen was the subject of an important communication by Romanes (19). The method adopted consisted in sealing up seeds in vacuum-tubes of high exhaustion, in which they remained fifteen months; in other cases, after rest of three months in vacuo, seeds were transferred to other tubes charged respectively with various gases at the atmospheric pressure of the time of sealing. After a further period of twelve months the seeds were sown. The gases employed were carbon-dioxide and -monoxide, oxygen, hydrogen, nitrogen, hydrogen-sulphide, water, ether, chloroform. In all cases (with one exception) the seeds were weighed individually, and seeds of equivalent weight were taken from the same original packets as controls. The results proved amply, that neither a vacuum of 1/1,000,000 of an atmosphere, nor any of the gases, exercised much, if any effect on the 'vitality' of the seeds. Germination was in all cases perfectly normal. These experiments prove conclusively that dormant seeds do not respire.

Confirmation of Romanes' results are not wanting; similar ones have been obtained by De Candolle (2), Jodin (5) and Ewart (9) as regards the effect of deprivation of oxygen. The first enclosed seeds in a capsule of metallic foil, to which a wire of platinum was attached; the capsule was plunged under mercury, and frequently agitated to remove bubbles of air adherent to the seeds. If respiration occurred, it could be only due to air contained in the seeds before immersion. The results of four experiments showed that this treatment did not affect the 'vitality' of the seeds. In two of Jodin's experiments seeds were introduced into a bent receiver full of air, which was inverted under mercury; the whole was kept in a dark cupboard, in one case for four years, seven months and six days, in the other for three years and seven months. The air was then

analysed and the seeds tested in the first case. In the first experiment the composition of the air had been practically unchanged; every seed germinated. In the second some oxygen had been absorbed and some carbon-dioxide formed, but the amount was small. In a third experiment twenty peas (*Pisum ?*) were introduced into a test-tube filled with mercury, and inverted over this liquid. No gas was noticed at the beginning or end of the experiment. After 4·5 years, ten peas were placed to germinate; eight developed perfectly, two refused. At the end of ten years and three months the remainder were removed; two germinated normally, two abnormally, and six not at all. Thus peas remained more than ten years in mercury-vapour of feeble tension without respiring, and yet preserved 'vitality' to a certain extent. For the study of this question Ewart placed seeds in corked bottles with an open erect tube containing clean pieces of sodium; the tube was then hermetically sealed. The atmosphere soon consisted of hydrogen and nitrogen. After three and five weeks the seeds were tested. Resistance, which was considerable, varied with the species. This experiment is also of interest, since it measures the resistance of the seeds employed to desiccation.

Van Tieghem and Bonnier (20) have obtained results discordant with those mentioned above. Samples of seeds were divided into three equal parts, each having the same number of seeds of the same species. One lot was placed in a cardboard box, into the interior of which air had free access; the second was placed in a glass tube full of air, which was then carefully corked and submerged under paraffin at the corked end: the third was put into a test-tube full of mercury which was then filled with carbon-dioxide. The seeds had been previously weighed. Each tube was submitted to like conditions for two years. All the seeds left in the open air had increased in weight by the close of this period; those in closed air had done so very slightly, whereas those in carbon-dioxide had not altered their weight. Germinating power was highest in the seeds left in the open air, much less in those kept in the closed air, whereas those previously in carbon-dioxide failed to germinate in every

case. The closed air had in the tubes of two out of four species suffered considerable alteration. The increase in weight of the seeds is attributed to metabolism. Since the authors tested only four species and made but few experiments with them, whereas Romanes made most careful trial of nine, and several series of experiments, it is more probable that the results of the latter are more accurate; further, they are confirmed by those of Ewart, De Candolle and Jodin, whereas those of van Tieghem stand isolated.

The action of vapour of ether, chloroform, alcohol and acetic acid on dormant seeds was also tried by van Tieghem and Bonnier. Seeds of five species were suspended in a flask containing the liquids; after introduction of the seeds, the flask was hermetically sealed. Exposure of four hours reduced the percentage of normally germinating seeds and retarded development; exposure of four days accentuated the results, while more prolonged action destroyed 'vitality'. The integuments were shown to afford enormous protection. The former results contradict again those of Romanes.

De Bary[1] has stated that seeds of *Lepidium sativum* withstand immersion of a month in absolute ethylic alcohol; Ewart (9) has confirmed this, but finds that 'vitality' is lost after exposure of five weeks. Peas (*Pisum ?*) endured immersion of seven to nine weeks without losing the power of revival in all cases; immersion of nine weeks however annihilated this. Seeds of *Cannabis sativa* lost the power in one day, those of *Linum usitatissimum* and *Triticum* (*vulgare ?*) by five weeks; those of *Hordeum* (*distichum ?*) by seven weeks. Fifty per cent. alcohol destroyed the 'vitality' of these seeds in one day; those of peas (*Pisum ?*) withstood 90 per cent. alcohol better than both preceding percentages. The seeds mentioned above lost 'vitality' after immersion of one day in aqueous solution of mercuric chloride (percentage not given); peas (*Pisum ?*) however withstood it in alcoholic solution from three to six weeks.

[1] *Lectures on Bacteria*, p. 63. Quoted by Ewart.

Experiments were also instituted to ascertain whether dormant seeds respired; air deprived of carbon-dioxide was aspirated over peas and then through an aqueous solution of barium-hydroxide for ten to fifteen days: no trace of carbonate was formed. This is a further refutation of van Tieghem's statements.

The action of twenty-two salts on the 'vitality' of dormant grains of *Triticum* (*vulgare ?*) has been studied by Bruttini[1]; the grains were immersed for twenty-four hours in 1 to 2 per cent. solutions: considerable difference in inhibitory action resulted.

Similar experiments have been made by Vandevelde,[2] who steeped seeds in various solutions for twenty-four hours and then placed them to germinate. Every solution influenced germination. He intends to publish shortly an extensive communication, embodying the results of his own work and that of others.

Mosso[3] finds that alkaloids exert a narcotic or stimulating action on germinating seeds according to the concentration of the solution employed. Seeds were germinated in solutions containing from 0·00001 to 2 per cent. of the hydro-chlorates of morphine, nicotine, cocaine, of the sulphates of strychnine and atropine, and of caffeïne salicylate.

Retrospect.—Exigence of space forbids more than a short critique of the more important questions raised on comparison of the preceding pages. One point towers above all others, namely, that of the 'latent life' of dormant seeds. Pictet has stated that no chemical action occurs at − 100° C., and Dewar has undoubtedly shown that it is unknown with one or two exceptions at from − 182° C. to − 192° C.; yet seeds kept for four consecutive days at − 100° C. germinated, and still more such as had been exposed to nearly − 200° C. Further, Romanes, Ewart and De Candolle have shown that seeds, that have not respired for more or less long periods, retain 'vitality'

[1] A. Bruttini, *Ann. Agron.*, 21, 1895, 542.
[2] A. J. J. Vandevelde, *Chem. Centralb.* 1897, p. 822.
[3] U. Mosso, *Ann. Agron.* 21, 1895, 544.

still, while JODIN proved that this is also true when respiration has been completely inhibited for years.

Are, then, dormant seeds alive, or are they dead?—The most perfect definition of life is probably that of Herbert Spencer,[1] who defines it as "the continuous adjustment of internal relations to external relations". Now dormant seeds may be subjected to most varied environment, the factors—temperature, hydration, time, etc.—undergoing enormous variation, and yet remain absolutely inert; there is no evidence of adjustment of internal relations to external relations. If this definition be accepted no life, whether latent or weakened (*ralentie*), can be attributed to dormant seeds. It is possible however that some may hold that, in the light of present knowledge, this definition becomes untenable, and that an extension is necessary. This amendment is not however practicable; for if Spencer's definition were so modified or extended as to comprehend the state of dormant seeds, all definitions of life must become self-contradictory, and consequently all definitions of protoplasm, which have for the most part but the one postulate of life in common, would then in addition become self-contradictory, for it would be impossible to combine in a definition the qualities of the cell-contents of a resting seed, on the one hand, with those of protoplasts on the other, since they are perfectly antithetic. It must be hence granted that dormant seeds are not alive.

Are they then dead? — Weismann's[2] definition of death, "der unwiderbringliche Verlust des Lebens eines Organismus," is excellent, provided that the nature of life is fixed. By substituting Spencer's definition of life for the word life, two harmonising definitions of life and death are obtained, which are probably more perfect than any others. Now it is a truism that many dormant seeds germinate when placed in suitable conditions,—hence, these are not dead.

What state then is this, that is neither life nor death?—As a matter of fact life in all dormant seeds is completely

[1] *Synthetic Philosophy; Principles of Biology*, London, 1884, vol. i., p. 80.
[2] Aufsätze über Vererbung, Jena, 1892. *Ueb. Leben und Tod.*, p. 131.

arrested; in some cases potential life is retained, in others it is lost. For the sake of illustration the former state may be compared with that assumed according to Buddhistic philosophy by an individual consequent upon his entering Nirvâna; once here all individuality is annihilated: the state is existence without a single attribute of life. There is however one essential difference between this state and that of the dormant seed; an individual after entrance into Nirvâna has lost even potential individuality, but the seed retains this. In the case in which seeds have lost potential life, death obtains. It is at present impossible in most cases to distinguish between these two kinds of seed in the naturally desiccated condition, no matter what agents be invoked, except by testing the power of germination. That there is a chemical, probably also a physical, difference is obvious; the difference may be only slight, but it suffices to determine whether resuscitation is possible or not.

It seems physiologically necessary to distinguish verbally between this state of dormancy and that of life, on the one hand, and death on the other. This can be adequately done—as I venture to suggest—by the term hypnosis; the corresponding adjective is hypnotic, and the substantive, hypnote. The cell-contents of a hypnote would then be hypnoplasm; hypnospore and hypnosperm are already recognised phytological terms, and experiments of Pictet have justified their existence, for he has found that these spores are still hypnotic after exposure to a temperature at which no chemical action is possible. It is true that some of these terms have a different connotation in psychology, but this need not militate against their employment in phytology. The homologue of hypnoplasm in dead seeds, etc., may be termed necroplasm, corresponding with the established term necrosis. Bioplasm as an equivalent for protoplasm has long ago been coined by Beale.

The protoplast has often been compared with a machine; this simile has been as often declared inappropriate. De Candolle (2) has indeed shown that, as regards ordinary machines, this is an inefficient comparison; the same has

been more completely demonstrated by Spencer.[1] It is distinguished by one essential quality, irritability, from all known machines. But, on the other hand, the protoplast as a phenomenal object can only be an instrument for transmission and transformation of energy, that is, a machine. How can this apparent paradox be explained?—The solution is not difficult. The term machine is always used to denote an instrument for transmission and transformation of energy, the nature of which is more or less perfectly known; but in the protoplast processes occur concerning which practically nothing is known. It is these difficulties that form the stumbling-blocks over which so many fall; inability to appreciate the protoplast as a machine is merely a phase of mysticism. The phytologist that refuses to recognise the protoplast as a machine errs similarly to the savage who attributes life to a watch, because the mechanical processes manifested in this machine are not recognised by him as identical in essence with those of more simple machines with which he is acquainted. The one makes an animal out of the watch, the other a god out of the protoplast.

It is not to be inferred that a form, or forms, of energy differing from any at present known may not be manifested in the protoplast; that may be so, or not. All that can be now said on the subject is: That the protoplast is a highly complex machine, and moreover an automaton, differing from all known machines, and that the rationale of its functionation is for the most part unknown. Pictet (15) says with perfect justice that life is a manifestation of natural laws, of the same type as gravitation. But he errs in stating that life is one of the constant forces of nature, if he intends to denote thereby that life is a single action comparable with gravitation; this may be so, but present knowledge is too insufficient to allow even condonation of such dogmatism.

Thus the protoplast is an automaton in action, the hypnoplast one at rest, and the necroplast one, the organisation of which has suffered irreparable injury. Also,

[1] *Loc. cit.*, chaps. iv., v.

organisms can assume one of three states—biosis (life); hypnosis (trance): necrosis (death).

The evidence with regard to 'longevity' in hypnotic seeds is very incomplete. Giglioli has suggested that these, after perfect desiccation are immortal; this is perfectly unwarranted, for, up to the present, no seed—as far as I am aware—has been ever completely desiccated. That ordinary hypnotic seeds with the average determined aqueous content of 10 to 12 per cent. are not immortal, is a truism. JODIN explains loss of hypnosis by intramolecular changes, analogous to those which occur in amorphous substances on becoming crystalline in lapse of time, without the aid of external energy. There seems no objection to this view.

In conclusion it may not be considered highly irrelevant to deprecate most strongly the laxity displayed by many phytologists in their methods of nomenclature. Plants are frequently specified as peas, beans, etc., a procedure that detracts very considerably from the value of their work. A pea, for example, may belong either to the genus *Pisum*, or to *Lathyrus;* also, a bean may either belong to *Vicia (Faba)* or *Phaseolus.* Such terms hardly beseem pages of a modern horticultural periodical, *a fortiori* those of a scientific one.

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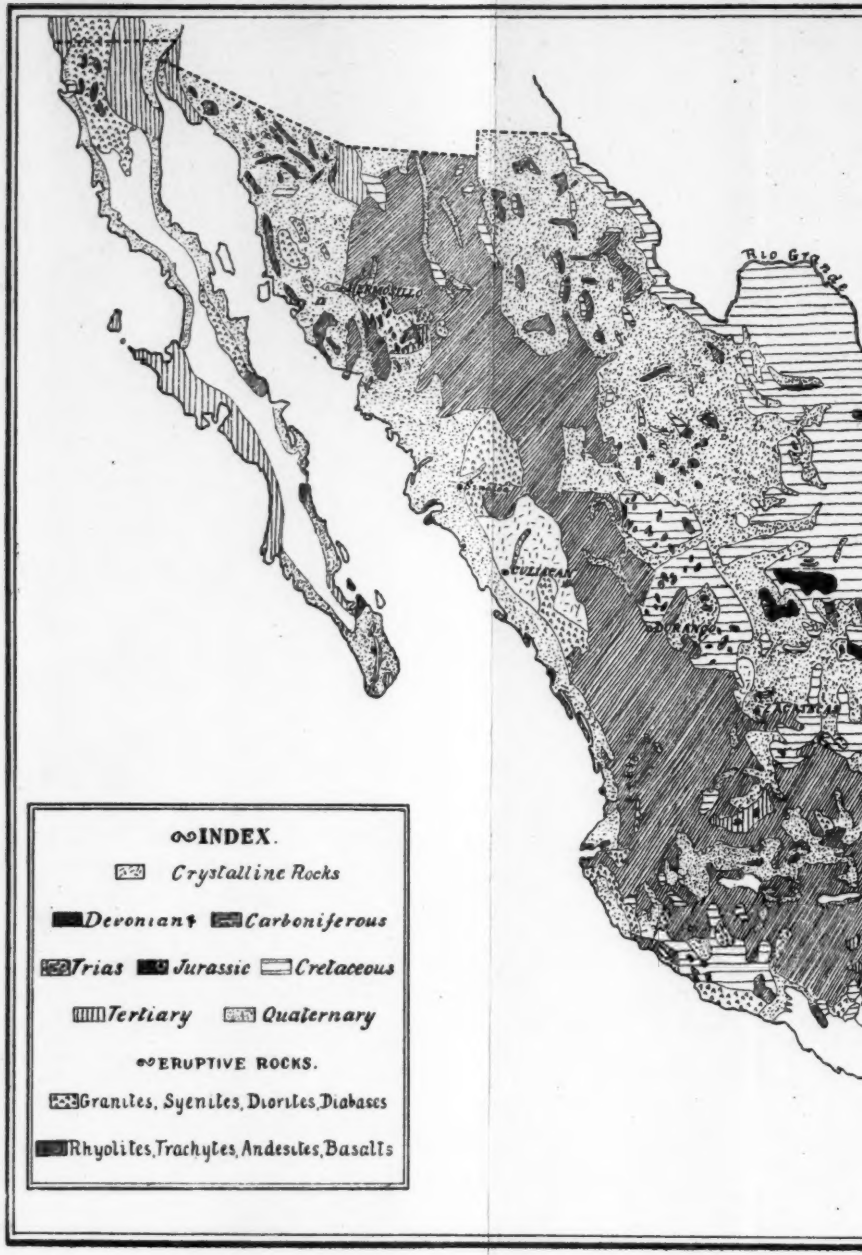
F. ESCOMBE.


Rio Grande
HERMOSILLO
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INDEX.
Crystalline Rocks
Devonian
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Rhyolites, Trachytes, Andesites, Basalts

Vera Cruz

THE GEOLOGY OF MEXICO.[1]

IT is an encouraging sign of the times that the Republic of Mexico has recently established a geological survey; and it is still further matter for congratulation that the survey has already proceeded so far that it has been able to issue a sketch of the geology of the country as a whole. The sketch, no doubt, will require considerable alteration in the future; and, indeed, it is apparent upon the face of it that the palæontological evidence has not yet been sufficiently examined. But there is no reason to doubt its general accuracy, and it gives a very good idea of the geological structure of the Republic.

In a broad and general way the geology of Mexico offers some resemblance to that of the Peninsula of India. There is the same almost entire absence of fossiliferous Palæozoic rocks. The Trias contains little but plants, and the Jurassic is very feebly developed. At the close of the Cretaceous period there began a great series of volcanic eruptions, comparable with the Deccan Trap; but in Mexico these eruptions have continued down to the present day. In Mexico, too, the marine Cretaceous is much more extensively developed than in India.

Geographically, Mexico consists of a great central table-land, inclined towards the north and north-east, and included between two chains of mountains (the Sierra Madre del Pacifico and the Sierra Madre Oriental), which are separated from the sea by narrow strips of low-lying country. In the southern part of the republic the two chains unite with each other in the form of a V, near the apex of which are situated the great volcanoes of Popocatepetl, Ixtaccihuatl, Orizaba, etc.

South of the V, the country is lower, with a range of mountains parallel to the Pacific coast.

[1] Jose G. Aguilera. "Sinopsis de geología Mexicana. Bosquejo geológico de México: segunda parte." *Bol. del Inst. Geol. de México.* Nums. 4, 5, y 6, pp. 189-270. One map. Mexico, 1897.

Geologically, the rocks which occur in Mexico, may be divided into three distinct groups. First, we have the ancient crystalline and schistose rocks, which are presumed to be of Archæan age; secondly, the sedimentary deposits, which extend from the close of the Palæozoic era to the present; and lastly, a great series of volcanic rocks, which are mostly of Tertiary and Quaternary age.

The crystalline rocks constitute a great mass, which occupies a large part of the country at the southern angle of the V formed by the great mountain chains. They extend also for some distance along the western coast, and form a considerable part of the hills of the Sierra Madre del Pacifico in the state of Sinaloa.

The sedimentary deposits are much the most extensive of the three groups. They cover the northern, central, and eastern parts of the country, and also the southern extremity. None have been proved with certainty of earlier date than the Carboniferous, and neither these nor the Trias nor Jurassic are found except in isolated patches. By far the greatest space is occupied by Cretaceous rocks, which form nearly the whole of the Sierra Madre Oriental, and also a large part of the central plateau, where, however, they are often concealed by Quaternary deposits.

The volcanic rocks form nearly the whole of the Sierra Madre del Pacifico, and in the latitude of Mexico and Puebla they stretch almost across the width of the country.

ANCIENT CRYSTALLINE AND SCHISTOSE ROCKS.

The crystalline rocks, as already explained, form a large nucleus which occupies the southern part of the state of Puebla, some portions of the Sierra Madre in the state of Chiapas, and extensive areas in Oaxaca and Guerrero. Small scattered patches are also met with in the central plateau and elsewhere, while there is a large mass in Sinaloa. Crystalline rocks also form the axis of the mountain chain of the Californian Peninsula.

The rocks consist of porphyroid gneiss, gneissic and argillaceous schists, penetrated by veins of granite, granu-

lite, pegmatite, etc. Since no fossiliferous beds are known of earlier date than Carboniferous, it is evident that the age of these crystalline and schistose rocks is at best very uncertain.

PALÆOZOIC.

Silurian.—In the collection of the Institute there is a fragment of limestone displaying very fine specimens of *Orthis testudinaria*, Dalm; and this is stated to come from La Cuesta de Santa Teresa, one league (about two miles five furlongs) east of Cacahuamilpu, in the state of Guerrero. Several expeditions, however, have been sent, without success, to discover these deposits, and the occurrence must be considered doubtful.

Other rocks of various ages have been placed in the Silurian by various writers, but the correlation is in no case based on satisfactory palæontological evidence.

Devonian.—In the same collection there is a fine example of a Goniatite, which is referred to the Middle Devonian. The specimen is stated to have come from the ridge of Apulco; but here again subsequent expeditions have failed to discover any further specimens. It is believed, however, that certain schists below the Jurassic rocks of this neighbourhood may have yielded this fossil, and may really be of Devonian age.

Carboniferous.—The occurrence of Carboniferous rocks in Mexico has been definitely proved. They are found on the borders of Guatemala, where they contain *Productus semireticulatus.* The rocks are limestones and dolomites, passing down into sandy conglomerates.

Dr. Persifor Frazer has referred to the Upper Carboniferous the limestones which form some of the ranges in the states of Coahuila, and Nuevo Leon; and he bases this view on the identification of a certain number of fossils by James Hall and A. Heilprin. But the specimens are too badly preserved to be specifically identified, and even the generic determinations are open to doubt. From the results of other expeditions to these localities, the limestones are now believed to be the Cretaceous.

Other rocks which have been referred to the Carboniferous, have since been found in some cases to be of Cretaceous, and in others of Triassic, age.

MESOZOIC.

Trias.—The Trias appears at one time to have covered a considerable extent of country; but it is now visible only in a few scattered patches, of which the best known is perhaps that of Los Brancos in Sonora. The formation is also met with in Puebla and other states.

The beds consist of quartzose sandstones and argillaceous deposits. They are very variable in character and change not only vertically but also horizontally.

Ceratites has been recorded from the Sierra de la Encantada, in the state of Chihuahua; but this range is stated by Aguilera to consist of Cretaceous deposits.

The fossils which have been found are chiefly plants, and among these are species of *Gangamopteris* and *Macroptæniopteris*, genera which are characteristic of the *Glossopteris* flora of India and the southern hemisphere.

Jurassic.—The Jurassic rocks also are met with only in scattered patches, the largest of which occurs on the borders of Zacatecas and Coahuila. They consist of argillaceous marls and marly sandstones, covered by calcareous marls, and finally by limestones, which pass up quite conformably into the Cretaceous.

Fossils are abundant, but have not yet been closely examined. They include a considerable number of Cephalopoda. Most of the species are held to be distinct from those of Europe, but the most abundant are of Upper Jurassic facies.

Cretaceous.—The Cretaceous System, unlike the preceding, occupies a very considerable area. It forms the greater part of the Sierra Madre Oriental and covers most of the great central plateau, excepting where it is concealed by Quaternary deposits. It also occurs in large patches in the southern part of the Republic, south of the great plateau.

Palæontologically and lithologically it may be divided into three great subdivisions.

The Lower Cretaceous is composed of green argillaceous and marly rocks and calcareous and marly sandstones, covered by variegated clays. Sometimes it rests directly upon the crystalline rocks and sometimes upon the Upper Jurassic.

The Middle Cretaceous consists of grey, blue, or blackish limestones, which occasionally emit a fœtid odour when struck with a hammer. This is much the most widely spread of the three subdivisions. It predominates in the north and east of the country and forms the greater part of the Sierra Madre Oriental. Sometimes it rests upon the Lower Cretaceous, sometimes upon the Upper Jurassic, and sometimes upon the Trias; and it has suffered much from folds and faults.

The Upper Cretaceous consists of light-coloured sandstones alternating with grey and blackish argillaceous deposits, which locally become more or less marly or calcareous. This division is not so much affected by folds and faults as the preceding, and frequently lies horizontally.

Towards the close of the Cretaceous period, or at the beginning of the Tertiary, began the great volcanic eruptions which form so striking a feature in the recent geological history of the country.

The fauna of the Cretaceous is remarkably rich and includes a considerable number of European forms. *Hippurites*, *Radiolites* and allied genera are met with; and Ammonites are abundant. The fauna of the middle division (which corresponds with the Cenomanian, Turonian, and a part of the Senonian) is naturally the most abundant, since this division is the most widely spread.[1]

TERTIARY.

The Tertiary deposits are found along the sea-coasts and also on the central table-land. In the former case they are marine, and in the latter lacustrine.

[1] I have not referred hitherto to the wealth of minerals in the various systems; but it is worthy of note that the Cretaceous Limestones are in places impregnated with cinnabar and other ores of mercury.

Among the oldest of the Tertiary rocks are those which succeed the Cretaceous in the valley of the Brave (the Rio Grande) between Laredo and Guerrero. They consist of sands and marls, and belong to the Lower Eocene; and they are succeeded by marly sandstones which belong in part to the Eocene and in part to the Miocene.

In the Peninsula of California, especially upon the Pacific side, the coast is formed of sandstones and conglomerates, which consist largely of fragments of trachyte and andesite. These belong to the Upper Miocene and Pliocene.

In the low-lying country along the coast of the Gulf of Mexico, the Tertiary deposits form a band parallel to the shore, extending from the Rio Grande southwards beyond Vera Cruz, where they expand into a broad mass covering the greater part of the states of Tabasco, Campechi, and Yucatan. These deposits are marine in character and consist of shelly limestones and agglomerates. In the lower beds Miocene fossils predominate, while in the upper beds Pliocene and living forms are common.

Besides these, there are other deposits which must be referred to the Tertiary period. Thus, the lignite-bearing beds of Zacualtipan, in the state of Hidalgo, may be placed at the top of the Miocene or in the Pliocene; and numerous conglomerates formed of Cretaceous and other fragments are believed to be of Pliocene age.

But the most interesting of the Tertiary rocks are the volcanic lavas, agglomerates and ashes which cover so much of the country. The period of vulcanicity began with the ejection of syenites, hornblendic diorites, quartz diorites, and diabases, the age of which cannot be exactly determined, but is believed to be either late Cretaceous or early Eocene.

The next eruptions, which belong to the Miocene period, consist of andesitic porphyrites, and propylitic andesites; and then followed, in order, hornblendic andesites, hypersthene andesites, augite andesites, and labradorites. The last of these are evidently the precursors of the great eruptions of basalt, which began at the close of the

Pliocene and reached their maximum in the Quaternary period.

Rhyolites and trachytes, however, also occur.

The fossils of the Tertiary are very abundant, and include both marine and terrestrial forms. Among the latter may be mentioned *Hippotherium* and *Protohippus*.

QUATERNARY.

Quaternary deposits occupy a wide area in Mexico. They cover a large part of the central table-land, and, in a greater or less degree, the slopes of the mountains and the floors of the valleys. But, excepting the great lava-flows of the period, which are basaltic in character, they do not offer any special points of interest.

PHILIP LAKE.

BLOOD AND THE IDENTIFICATION OF BACTERIAL SPECIES.

THE identification of any particular bacillus may be either a very easy or a very difficult matter. The detection of tubercle bacilli in sputum can be pretty safely entrusted to any conscientious medical student who has done his course of practical pathology: the determination of typhoid bacilli in the excreta of a patient may require all the resources of an experienced bacteriologist. During the last two years, however, the development and proper understanding of previous isolated or misinterpreted observations has given us a new and generally trustworthy method of recognising many species of micro-organisms. And more than this, by a simple reversal of the procedure it has been possible to supply a new means and aid to clinical diagnosis—another addition to practical medicine derived from experimental science.

So for back as 1889 Charrin and Roger (3) demonstrated that the growth of *B. pyocyaneus* (and of other bateria) differed when made in the serum of animals immunised against *B. pyocyaneus* and when made in the serum of normal animals. The differences consisted in (i.) serum remaining more transparent, (ii.) formation of granular agglomerations and (iii.) microscopically, the formation of chains of altered bacteria, in the former mode of cultivation.

In 1891, Metschnikoff, whilst investigating *Vibrio Metschnikovi*, saw the same phenomena, and noted them as facts requiring further investigation (10). But he made no progress in this direction, being discouraged by failure to obtain the reactions with the bacillus of hog cholera.

In 1893 Issaeff made similar observations with Pneumococci, and later in conjunction with Ivanoff, with *Vibrio Ivanovi.* They attributed the effects obtained to the bactericidal and growth-hindering substances in the serum (8).

In 1895, Washbourn saw similar appearances, and remarked that the mode of growth in the serum seemed to give a good indication of its protective power (13).

All these observers appear to have worked with undiluted serum only, and that too acting for a considerable time. Although they examined the deposit with the microscope they do not appear to have investigated microscopically or otherwise, either the immediate effect of the serum on the bacilli, or the effect of diluting the serum.

In 1894 R. Pfeiffer published what is now known as Pfeiffer's reaction (11), but it was Bordel who, in 1895, trying to simplify Pfeiffer's reaction, first described any observations in which the above-mentioned neglected aspects of the question were considered. He saw the agglomeration and loss of movement, but failed to attribute any specific importance to the reaction (*2a*).

It was therefore left to Durham and Gruber, who had already been more than six weeks at work on the subject when Bordet's investigations were published, to show how valuable and characteristic this reaction could be made, and to establish its general applicability (*4a*, *6a*).

In March, 1896, I applied the reaction to the diagnosis of enteric fever in man, but scarcity of material and other delays prevented any paper on the subject appearing before September (*7a*). Meanwhile Widal, profiting by a hint thrown out by Professor Gruber at the Wiesbaden Congress (*6c*) in April, and having more cases at his disposal, was able to publish a communication on serum-diagnosis early in July[1] (*14a*). Pfeiffer's reaction is observed by injecting

[1] Professor Widal has persistently ignored the dates given in my articles and constantly represents me as having merely confirmed his results. As a matter of fact, so far as I am concerned, the invention of serum diagnosis was made independently, possibly earlier, and arrived at from an opposite point of view. On the other hand, there are certain points, *e.g.*, (i.) action of serum from other than enterica patients, (ii.) action of human serum on bacilli other than *B. typhosus*, (iii.) variability of strength of serum in enterica patients from day to day, (iv.) difference of sera of mother and child, and (v.) desirability of accurate dilution, contained in my papers; these Professor Widal has incorporated and repeated without acknowledging their origin.

a mixture of cholera vibrios together with a small quantity of serum from an animal immunised against cholera into the peritoneal cavity of a normal animal. By withdrawing drops of fluid from the abdomen at different intervals it can be seen that the micro-organisms first lose their motility, then become transformed into spherules and finally disappear. The rapidity and completness of the reaction depends on various factors (*e.g.*, potency of serum, extent of dilution, etc.). It is not so easily performed with typhoid bacilli.

Pfeiffer thought vital action essential, but Bordet found that the same phenomena could be observed outside the body if the temperature were kept at 37°C. He generally performed the reaction with a mixture of bacilli, "immune"[1] and normal serum, but he reccgnised that it could occur well with only "immune" serum, and also, although generally more feebly with only normal serum from various sources. Bordet laid chief stress on the granular degeneration of the micro-organisms rather than on the agglomeration into groups.

Durham and Gruber examined the phenomena much more thoroughly, and extended it to several kinds of micro-organisms. They found that the addition of a small quantity of "immune" serum to an emulsion of the corresponding bacillus in a test tube caused a precipitation of the bacilli in the form of fine granules, the supernatant liquid becoming clear. In fact the reaction resembled an ordinary chemical reaction occurring slowly. A control tube containing only bacilli would remain uniformly turbid. The rapidity and completeness of the reaction were found to depend on the same causes as in Pfeiffer's reaction. If the same experiment is performed with smaller quantities so that the process can be observed microscopically, under suitable conditions the following stages are seen. The bacilli at first isolated and moving rapidly about and across the field gradually slow down and begin to hang on one

[1] For the sake of brevity, "immune," "typhoid," "cholera," etc., serum will be used for the serum of an immunised animal, for the serum of an animal immunised against cholera, typhoid, etc., respectively.

to another; groups of three or four are thus formed which then adhere to other groups, until, if the reaction be complete, all the micro-organisms are collected into large clumps, with total loss of movement. Should the serum be very potent and not sufficiently diluted, agglomeration and loss of movement may occur instantly, so that by the time the slide is brought under the microscope the reaction is already completed. On the other hand, with a weak or much diluted serum, the phenomena may remain permanently incomplete, so that several isolated micro-organisms are still seen, and even some of those in the clumps may retain their motility. So delicate, however, is the reaction that serum from a very highly immunised animal will produce an effect on its corresponding bacillus even when diluted 500,000 times. The action is to a considerable degree specific (or as Durham prefers to term it, special), for a "cholera" serum will not act on typhoid bacilli, and "typhoid" serum will not act on coli bacilli. On the other hand, "cholera" serum will affect to some extent certain vibrios which are not cholera, and "typhoid" serum will agglomerate *B. enteriditis*. But there is generally some difference, and any given serum acts best, *viz.*, in greater dilution, on its own kind of bacillus, so that it can be used for its identification, without causing confusion.

The extent to which any serum can be diluted and yet show an effect depends in part on the virulence of the culture employed; the more attenuated the culture, the weaker the serum solution necessary to produce an effect. It is to this grouping power that Durham gave the name "clumping action" of the seurm, and the hypothetical body producing it was termed by Gruber "agglutinin".

It is obvious that the reaction can be applied to the identification of various kinds of bacilli, if we possess specimens of the corresponding "immune" sera; and perhaps, as will be indicated later, it may be of use in the discovery of new forms. In spite, therefore, of the sources of error just mentioned, a "typhoid" or "cholera" serum can prove of great use in determining whether doubtful organisms belong to these species. It is also clear that

the reverse will be true : that by means of a known bacillus we can identify an unknown serum, and can with comparative ease ascertain whether an animal is at all, or to what extent, protected.

That the "agglutinin" is distinct from the bactericidal and paralysing substances in "immune" or other serum appears from the following considerations. An "immune" serum exhibiting strong agglutinative power need not be appreciably more bactericidal than normal serum. A normal serum may exhibit agglutinative without paralysing action (*7a*). Agglutinins are not produced in the serum of animals immunised against certain micro-organisms ; *e.g.*, *B. diphtheria* and certain micrococci.

Motility of the organism is not, as was at first supposed, essential for agglomeration. Not only can agglutination occur using dead, motionless bacilli instead of active living ones (*2b*), but it may be seen also with some non-motile bacteria and cocci. With some cocci I have seen a phenomenon resembling the loss of movement of motile bacteria. Before the addition of the serum, the cocci usually exhibit a Brownian movement which is much restrained or even entirely inhibited by the action of suitable serum (*7b*).

Gruber explains the phenomenon of agglomeration by the supposition that the agglutinin causes the enclosing membranes of the bacilli to swell out and become sticky, and that they consequently adhere to each other when they come in contact. He also put forward the idea that the effect of the special agglutinin is thus to facilitate the action of the general bactericidal substances common to all sera ; and his theory of immunity is based on these two fundamental ideas.

The theory has not been unanimously accepted. Pfeiffer considers each serum to have a strictly specific action. Salimbeni does not believe agglutination to occur inside the body (12). Nobody has been able as yet to demonstrate a swelling of the capsule. Be all this as it may, the observed facts, without any theory, form a valuable addition to bacterial knowledge.

The "active" immunisation of animals is usually attained

by the injection, either hypodermically or into the peritoneal cavity, of dead or attenuated cultures of the micro-organisms. The destruction of the culture used may be effected by the use of either heat or disinfectants, but too great an application of either diminishes or destroys the immunising power of the injection (14*b*). The date on which the reaction first appears after injection is rather variable, and depends on two or three factors, although chiefly on the strength of culture. Thus a guinea-pig which I injected with living cholera culture on 18th April, 1896, showed the reaction well on the 21st, whilst one injected with dead cholera culture on 16th March. 1896, did not exhibit marked agglutinative action till 23rd March. It is not even necessary to inject the bodies of the bacteria: filtered cultures give similar but weaker or more delayed results (14*b*, 9). By the simultaneous injection of two kinds of micro-organisms agglutinins for both can be produced in the same serum (14*b*).

"Passive" immunisation is generally produced by the injection of serum from an actively immunised animal. In such a case the agglutinative property shows itself much sooner. Thus, in a guinea-pig which I injected at 3 P.M. on 9th March, 1896, agglutinin could be detected in the blood already at 3·30 P.M., and had attained its maximum strength about 6·50 P.M., remaining at the same strength for about one week afterwards. An actively immunised guinea-pig generally possesses agglutinative serum for a considerably longer period.

The nature of the agglutinating substance has not yet been distinctly ascertained. Filtration of the serum through a porcelain filter diminishes its power. Precipitation by neutral salts of the globulin of the serum or the fibrinogen of the plasma causes loss, or sometimes only diminution, of agglutinative power in the filtered fluid. Similarly serum is generally only two-thirds as powerful as plasma, so that the agglutinin seems to be in some way bound up with the proteids of the blood. Leucocytes, when shed, do not apparently give off any agglutinin. Temperature affects agglutinin in proportion to its height and the time it is

allowed to act. Ten minutes at 75°C. destroys the substance, and an hour at 57°C. causes a diminution of effect. Merely heating to 60°C. does not deprive serum of its power (1, 14*b*).

What has been said hitherto relates nearly entirely to animals, and, of these, to guinea-pigs. We shall now consider the question mainly in relation to man.

The serum of normal guinea-pigs rarely exhibits agglutinative power, but that of some other animals, particularly of the horse, sometimes show it in a marked but varying degree. So, too, does the serum of man. The blood of many individuals possesses a pronounced agglomerating action on several forms of micro-organisms; but the quantity of each agglutinin is small, not showing itself when the serum is diluted more than eight times (7*a*). Thus normal human serum presents some striking differences from that of immunised guinea-pigs, principally in its multiple action. We shall consider later whether this is due to the presence of a common agglutinin or small quantities of each special agglutinin.

When normal serum has any agglomerating action it is often nearly equal on at least three kinds of bacteria—*B. typhosus*, the cholera vibro and *B. coli communis*—and its action on each disappears at about the same degree of dilution. In one diseased condition marked by the evident presence of abnormal substances in the serum, namely jaundice, the agglutinating action is always present; and if the serum be not diluted, it is as rapid and complete as with the serum of an immunised animal. Neither normal nor icteric serum, however, possesses the power of inhibiting movement. For this reason it seems probable that during the process of immunising there are formed in the specific sera two substances, one to cause the agglomeration and another to inhibit movement, for this latter quality is nearly as characteristic of such sera as the former.

If this agglutinative property carry with it a proportionate amount of immunity then a considerable number of individuals are slightly immunised against typhoid, against cholera, and probably also against other diseases.

It cannot yet be considered settled whether this property is inherited or acquired. Experiments on animals seem to show possibility of inheritance (14 *b*), but if in these experiments the inoculations were made into the peritoneal cavity there is an obvious source of error. In the few human cases which I have been able to examine, the blood of the child was usually inactive although that of the mother was generally, sometimes even powerfully, active. In one instance where the mother had had enterica five years previously, the child's serum exhibited but very feeble agglutinative power. In one case where the child was born during an attack of enterica in the mother the serum was very active, but in another it was not so; possibly in the first case the child was also passing through an attack. If the specific agglutinins are inherited they should be found fairly frequently in the sera of new-born children, especially abroad, where a larger percentage of the population have had either enterica or cholera. According to my own observations, in Austria at any rate, a greater percentage of adult than infant sera possesses this property, tending to show that it is an acquired quality. It may be that a more extended series of observations would reverse these figures. Anyway, in light of these differences of hæmic properties, statistics on the relative immunity (if it exist) of the children of "typhoid" parents would be of interest.

It was natural to suppose, as the result of experiments on animals, that typhoid (or cholera) patients, who were thus passing through a natural immunising process, would produce agglutinins in their blood. Observation showed this supposition to be correct, and vaccination experiments on man have confirmed it. In the latter case agglutinins have been observed in the blood already on the second day after injection (15*b*). When the infection of typhoid takes place in the natural way owing to the smallness of the initial dose, the agglutinin is not generally found in the serum until two or three days after the nominal commencement of illness, *viz.*, about sixteen days after injection. This may be taken to be about the time when the contest between organism and micro-organism seriously begins. In cholera,

with its much shorter incubation period and rapid development, we should expect to, and actually do, find the reaction earlier.

If the normal agglutinin in the blood is of a non-specific character, then an attack of enterica produces an entirely new substance and does not add to what is already present, because the effect of the serum on cholera vibrios and coli bacilli remains the same, only that on typhoid bacilli is increased.

The rate of agglutinin production varies in different cases as might be expected, and there is also a remarkable variation from day to day in the agglutinative power of the serum. These variations can hardly be entirely due to experimental errors. In immunising animals I have often noticed that the agglutinative power seemed to increase irregularly. Moreover, it is known that the agglutinin is used up during its action on the bacilli, this being one reason for always employing a constant quantity of culture. Consequently, it appears possible that a rapid increase in the number of micro-organisms, which may easily occur if they light on some fresh suitable soil, would use up the available agglutinin and reduce the amount temporarily below what it had been the day before and may reduce it below the level of normal blood. And it explains why the most severe cases of typhoid may have comparatively little agglutinin in their blood.

By the aid, therefore, of a known culture of typhoid bacilli and by suitable dilution of the serum to be tested to avoid any error due to the presence of normal agglutinin, it is possible to diagnose enterica by the agglutinin contained in the patient's blood. It is obvious that the principles of serum-diagnosis apply to other diseases as well as enterica. The method has indeed been used for the diagnosis of cholera, Malta fever and glanders. In diphtheria and tubercle it has not yet been successful. It might very well be applied also to the diagnosis of pneumonia, erysipelas, appendicitis and influenza if required, since it is not necessary that the specific organism should be motile. From a case of scarlet fever I isolated a coccus which agglutinates with scarlet fever serum (*7b*).

One general precaution must be mentioned regarding the serum-diagnosis of any disease. Although absurdly obvious, neglect of it has already led into error; namely, that it is no good applying the test if injections of "anti-toxic" serum have been made and agglutinins thus artificially introduced.

Not only may the agglutinin reaction be applied to the diagnosis of disease already present, but is also likely to be of use in the discovery of specific micro-organisms as yet unknown, *e.g.*, small-pox, typhus, measles, etc. For this purpose bacteria must be isolated from the various tissues and their reaction with the serum of patients convalescent from the disease in question ascertained. Naturally, any positive result must be controlled with other sera, and by suitable dilution of the specific serum.

There is one source of fallacy against which it is necessary to guard. Some infectious diseases, *e.g.*, scarlet fever, and small-pox in its later stages, are nearly certainly cases of mixed infection, more particularly with the organisms of sepsis. Under such conditions more than micro-organism might give a reaction with the serum, and other criteria would have to decide which is the specific one. Partly for this reason I do not consider the coccus above mentioned to be the specific organism of scarlet fever.

Without Durham and Gruber's exact and complete investigation and extension of the older isolated observations, a valuable addition to both practical bacteriology and to practical medicine would have been, if nothing else, for some time delayed. The application of the serum test to disease in man is but the natural outcome of their work. Being now part of routine diagnosis it is past the stage when the name of any individual should be attached to it, but in the history of this new departure in diagnosis their names have the first claim to be mentioned.

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ALBERT S. GRÜNBAUM.

THE FAUNA OF THE GREAT AFRICAN LAKES.

IT has been known for some years from the character of the shells of the Gastropods that have been brought back by travellers and missionaries from the interior of Africa that the great lakes of this continent contain animals which are quite unlike anything existing in the true lacustrine faunas from other quarters of the earth. At the same time these lakes are so remotely inland, and so greatly elevated above the surface of the sea, that all possibility seems to be precluded of animals such as deep-water gastropods and jelly-fish being either carried into them through the air or making their way up the rivers which run out of them to the sea. To determine the nature and affinities of these animals, and to account, if possible, for their presence in the great African lakes, was the object of a recent expedition which I made to the interior, the ultimate destination of which was Lake Tanganyika. I was, however, enabled while *en route*, through the exceptional facilities given to me by Sir Harry Johnston, to spend some time upon, and to become acquainted with, the physiographical characters and the faunas of both Lake Shirwa and Nyassa.

The discovery of the Tanganyika fresh-water jelly-fish by Boehm in 1886, the character of the shells of gastropods obtained by Burton and Speke, and still more those brought back by the missionary Mr. Hoare from the same lake, are all so remarkable in themselves that a considerable amount of scientific interest has been roused concerning the real nature and origin of these singular lacustrine forms.

The mere existence of a jelly fish in Tanganyika is sufficient to suggest the idea that the fauna of Tanganyika must have been at some time contaminated by incursions of these animals from the sea, and it is the merits of this theory of *marine contamination*, if I may so call it, which I wish to discuss in some detail at the present time. I wish more especially to call attention to the amount and character

of the collateral evidence which can be brought to bear upon this question from sources other than that of the morphology of the lake animals themselves. For although at first sight it might appear an easy thing, having obtained these animals, to determine their actual affinities and thereby decide once for all whether they are marine or not, the exact reverse is actually the case. The more we know about these animals the more difficult it becomes to determine what their actual affinities really are, and recently quite a number of alternative hypotheses have cropped up. Thus it is said (1) that these animals are not marine at all, that they owe their present character to the oceanic conditions operating in a great lake; (2) that they may be a common feature of African lake faunas, and therefore simply a unique character of the African lake fauna as a whole; (3) that their peculiarities are due to the latitude in the environmental conditions of the lakes themselves; (4) that they are the remains of a prehistoric *fresh water*, possibly cretaceous stock, which has survived in some of the African lakes; and so on, till the only satisfaction felt in the contemplation of these solutions is the certainty that they cannot all be true.

It is my chief object in the present article to examine first the physiographical differences of the lakes themselves, so as to be able to form some sort of an estimate of the effect these differences are likely to have produced in the faunas of the different lakes, and, secondly, the actual facts, so far as they have been at present ascertained, regarding the distribution of the families and genera of the aquatic animals among the lakes themselves.

When the evidence relating to these two lines of inquiry has been collected it will be seen that the greater number of the foregoing hypotheses are clearly incapable of application to the problems they were made to solve, and that there remains a direct issue between the respective merits of only three. Lastly it becomes apparent, when attempting to decide which of these three residual hypotheses really explains the nature and origin of these remarkable African lake forms, that the evidence drawn from the morphology of these animals themselves alone has any weight.

The larger African lakes are for the most part situated in the tropics north and south of the Equator, the bulk of Nyassa lying between 10° and 14° south. This lake is 1500 feet above the level of the sea, some 340 miles from north to south, and from twenty-five to forty miles across. Its water is profoundly deep, soundings of more than 300 fathoms, no bottom, having been obtained over a great portion of its length. The water of this lake, therefore, goes below the level of the oceans on each side of the continent, and from its immense size it acquires all the superficial characters of a stretch of sea such as the Baltic or the English Channel. The fauna of such a lake is exposed to the full force of storms and currents and to the effects of heavy surf, while owing to its altitude the superficial layers of water are at a considerably lower atmospheric pressure than those of the lakes and rivers on or about the level of the sea. Nyassa, moreover, has a broad outlet to the ocean to the east, and the water of the lake is thereby kept peculiarly sweet and fresh. These conditions are very different from those obtaining in Lake Shirwa, which stands about the same level at a short distance to the south. Here the water is nowhere more than about fourteen or fifteen feet deep, the lake is small in superficial extent, filled with reed patches which effectually prevent all surf, and, lastly, it is without an outlet, and consequently the organisms living in it have to endure a continually increasing salinity as the water year by year dries up.

Lake Bangweolo, which lies between Lake Nyassa and Tanganyika to the north-west, is a broad, shallow basin of water which is fresh and pure, but it stands at an elevation of 3000 feet, its basin being really a slight depression in the interior plateau of the continent, and owing to its greater altitude, the average temperature is far lower than that of the Nyassa Valley to the south. Except that it is deep, Lake Mwero is under the same physical conditions as Lake Bangweolo. Tanganyika is at a considerably lower level, 2700 feet, but it is so shut in by hills that the average daily temperature of the air and of the water of this lake are higher than those of Nyassa, although, as I have said, the

latter is only 1500 feet above the sea. This shut-in character of Tanganyika, and its consequent high temperature, is witnessed in a still higher degree in the valley of Lake Rukwa, whose lofty coasts rise almost perpendicularly from the water's edge from 5000 to 7000 feet. Owing to this cause the Rukwa region is extremely hot, and as the lake is without outlet the water is extremely salt. Again the Victoria Nyanza forms a vast expanse of shallow water like an enlarged edition of Bangweolo. But the characters of Tanganyika are repeated to some extent in the Albert Edward, the Albert Nyanza and Lake Rudolph to the north and east.

It is thus seen that the conditions under which the faunas of the lakes exist vary a good deal from lake to lake. The lakes themselves moreover vary considerably in the directness or the indirectness of their connections with each other and the sea. Thus Nyassa flows directly south and east to the Zambesi and the east coast, there being only one great series of rapids, at the Murchison Falls, throughout the whole of the effluent river's course. The effluents of Lakes Bangweolo, Mwero and Tanganyika, on the other hand, all flow together down the Lualabwa to the Congo and the Atlantic Ocean. This series of lakes is now, and must have been for a prolonged period of time, entirely cut off from all contamination through incursions of all but the most active marine animals coming up the rivers from the sea, for the courses of both the Lualabwa and the Congo[1] are intersected by formidable rapids and falls.

The effluents of the Victoria Nyanza, the Albert Edward and the Albert Nyanza go north and eventually reach the Nile, and in this way it will be seen that some of the great equatorial lakes have water communications with the seas and oceans north, east and west. The upper reaches of these rivers are, however, effectually blocked, so far as migration from without inward is concerned, by successions of falls and rapids where the rivers pass downwards from the high interior plateau, and of these several river courses, that from the west, stretching from the Lualabwa and

[1] See Stanley's description of this region in his *Darkest Africa*.

eventually reaching Tanganyika through the Luakuga, is, and must for a long time have been, the most impassable of the lot.

It is thus apparent that the general physiographical characters of the African lakes, the nature of the water in them, as well as the characters of the effluents which flow out of them when such exist, are in no way similar, they are on the contrary remarkably diverse, the physiographical characters of the lakes being, in fact, far more unlike each other than any one who was unacquainted with the wide climatic variations induced by altitude in the tropics would be likely to suppose. Such diversity in the environments to which the faunas of the great African lakes must have been and are now exposed, would, I imagine, be regarded by all experienced naturalists as quite sufficient if this action was at all prolonged, to induce profound changes in the characters of the organisms which originally migrated into them; but I know of no method by which the effect produced by environmental difference can be foretold, all that can be done is to examine the faunas of the lakes themselves collectively, putting down those individual peculiarities they present, which do not appear to be produced by oceanic contamination, or some such entire difference of nature to the operation of this cause. It must however be borne in mind that the power of changed environment, to produce corresponding changes in animal organisation, is by no means unlimited, and certainly its effect in Africa is more often witnessed in the reduction of the number of genera in a lake, than in the production of new ones. Exceptional conditions appear to be adverse to fauna as a whole, and in no way productive of abundant variations. Change in their environment however is not the only factor which might, and probably has, operated in differentiating these African faunas one from another. As Darwin pointed out many years ago it is only necessary to run a fence across a cattle park to produce different breeds on either side of it, and the isolation of groups of animals to which this process is practically equivalent, has been carried to a high degree of perfection in many of the African lakes.

Carefully bearing in mind the above enumerated facts, we may now examine the actual distribution of animals amongst the African lakes so far as this distribution is at present known, attempting thereby in the first place to estimate how far the individual variations presented by the faunas of the different lakes may be due to differences of environmental condition, or to what extent they must be explained through the operation of some other cause.

The Molluscs of the individual lakes are the group which is at present by far the most widely known, and the Molluscs from different localities show specific differences, which are at once obvious to the most casual observer; in fact very little experience of the Molluscs of different lakes enables one to tell at a glance from which lake any particular species may have come. Thus the Paludinas, which abound in the most astonishing profusion in nearly all the African lake waters, whether salt or fresh, show all sorts of gradations in size, from the little variety found in the fresh water of Nyassa to the large angular species of Mwero, and lastly to the largest Neothaumas of Lake Tanganyika, which species increases in size from the south to the north of the same lake. What is true of the Paludinas is true of the other Molluscs. There are specific differences peculiar to each lake, but beyond this the differences between the lakes is generally apparent simply in the omission of more or fewer genera as we pass from the great lakes to the small. The larger lakes have invariably by far the greater number of different genera, not only of Molluscs, but also of fish, crustacea, sponges, and the like. Nyassa, which ranks third in actual dimensions, and which, owing to its enormous depth, probably contains more water than any other single African lake, has yielded about seventeen different genera of Molluscs, among which there are the following genera of Gastropods: Limnea, Isodora, Physopsis, Planorbis, Ampullaria, Lanistes, Vivipera, Cleopatra, Bythinia, and Melania. Except that of Tanganyika, the Molluscan fauna of Nyassa is the largest of any known African lake, and it will be seen from the above list of Gastropods that it is, so far as they are concerned, of an

entirely lacustrine character. But what is true of the Gastropods in this respect is true of the remaining orders and families. The lake contains those only which are usually found to be associated together in the fresh waters of all the other great continents. There are individual peculiarities in some of the species contained in these Nyassan genera which are quite sufficient to distinguish the Nyassan fauna from that of any of the other African lakes. But they are by no means so great as might have been expected if the diverse conditions which I have pointed out as obtaining in the different lakes have really been operating for a very prolonged period of time. Thus the character of the fauna of Nyassa is entirely that of an ordinary lacustrine series, and has undoubtedly been derived from a purely fresh water stock. There is no form known in this lake which has any marine attributes about it, and there is nothing to suggest in the least that the lake has at any time been in connection with the sea.

The fauna of Nyassa is, however, extremely important to the present inquiry in another way. The Mollusca found in this lake, together with the fish, crustacea, and so on, compose a list of genera which includes all those found in almost all the neighbouring lakes, and thus if we arrange the names of the Nyassan genera of Gastropods, for example, in a vertical column, and write the names of the principal African lakes horizontally along the top, it will be seen that the genera of Gastropods which have been found to inhabit the other lakes are those which are contained in the Nyassan list, more or fewer of them being represented as the particular case may be. Thus in Shirwa we have Vivipera, in Kela, Planorbis, and so on. In the Victoria Nyanza and the Albert Nyanza there are a larger number of these same Nyassan genera, and it will be seen at once from the table that the Nyassa list covers all those hitherto found in every African lake but one. This solitary exception—Lake Tanganyika—does, however, contain the Nyassa list in full, but above and beyond this list there are some seven or eight genera of Gastropods which, so far as is at present known, are found living nowhere else in the world.

The isolated group of Molluscs constituted by these seven or eight genera are, then, something *superadded* to the general African lake fauna, and all the members of this group seem to be strictly limited to the confines of Lake Tanganyika alone. The evidence on this last point, however, is, as will be seen from the appended table, not yet complete, but it is, I am convinced, sufficiently complete to show that this group of Tanganyika Molluscs is not represented in any of the lakes in which Molluscs have hitherto been recorded. They are certainly not present in Nyassa, Shirwa, Bangweolo, or Kela; and the members of the above group are so conspicuous and common when they do occur, that they would certainly have been recorded from the Victoria Nyanza and the other members of the northern group of lakes if they had existed there.

Gastropod Genera occurring in the African Lakes.	Nyassa.	Shirwa.	Kela.	Bangweolo.	Mwero.	Rukwa.	Tanganyika.	Victoria N.	Albert Edward.	Albert N.	Baringo.	Rudolph.	
Limnea	–			No gastropods with shells according to Weatherly.	–	Not known.	–	–	–	–		Not known.	Fresh-water group common to all the lakes.
Isidora	–				–		–	–	–	–			
Physophisis	–				–		–	–	–	–			
Planorbis	–		–		–		–	–	–	–	–	–	
Ampullaria	–				–		–	–	–	–			
Lanistes	–				–		–	–	–	–			
Vivipara	–				–		–	–	–	–			
Cleopatra	–				–		–	–	–	–			
Bithynia	–				–		–	–	–	–			
Melania	–				–		–	–	–	–			
Neothauma							–						Marine group in Tanganyika only.
Paramelania							–						
Typhobia							–						
Syrnolopsis							–						
Lythoglyphus							–						
Lymnotrochus							–						
Undescribed Genera in Tanganyika							–						

Table showing the distribution of Gastropod Genera in the principal African Lakes.

There is, then, not a shadow of evidence to support the idea that the remarkable forms of Molluscs recorded from Tanganyika are a characteristic feature of the African lake

faunas as a whole; and this isolation of a number of Molluscan genera in Tanganyika is also shared by a number of fishes, crabs, prawns, sponges, medusæ, and several forms of protozoa, none of which are represented in any of the other lakes.

It becomes in this way gradually apparent that the fauna of Tanganyika is not only more profuse, but far more complex than that of its sister lakes. The fauna of Tanganyika is in fact naturally divisible into at least two distinct types or groups, one being made up of genera found to be widespread in all the other lakes, the characteristics of which are those of a typical fresh water stock; the second made up of a number of animals peculiar to Tanganyika, and the *tout ensemble* of this latter group is marine.

The broad facts of the distribution of animals among the great African lakes lead then to this most remarkable result, that in the one lake having an outlet at all, which at present is the most inaccessible from without, there exists to-day a group of animals found not only in no other lake, but the members of which are not quite like any other species, either salt water or fresh.

Now, if the foregoing statements have been borne in mind it will very soon become apparent that this curious result effectually disposes of the first three hypotheses which were given on page 628 as possibly accounting for the existence of these animals in African lakes. The animals in question cannot be a common feature in African lake faunas, since they appear to be exclusively restricted to the confines of one lake. They cannot have resulted from the effects of oceanic conditions operating in a great lake, because these conditions are quite as much pronounced in Nyassa and the Victoria Nyanza as in Tanganyika, and if it is suggested that Tanganyika may be older than the other lakes, and that its fauna may thus have had time to modify there more than elsewhere, I would point out in the first place that the normal fresh-water faunas of the other lakes are quite as much modified as that of Tanganyika, and further that there is more direct geological evidence for the antiquity of Nyassa than for

that of Tanganyika.[1] Lastly, they cannot conceivably have originated in Tanganyika through the effect of environment, since this would have affected the whole of the fauna and not simply have superadded new genera to a list which in other respects is so strikingly similar to that found in Nyassa.

There remain then three hypotheses, two of which if they are not still actually tenable must receive some further consideration here. The first of these seeks to explain the existence of the unique group of organisms found in Tanganyika, by supposing them to be the persistent remains of an extinct fresh water fauna, and this view has been said to have a certain amount of palæontological support. Some of the original shells brought home from Tanganyika have for several years been regarded by many geologists, notably by White[2] and Tausch,[3] as being probably related to certain Cretaceous brackish or fresh-water fossils of America and Southern Europe, the so-called Paramellanias of Tanganyika being compared to the Pyrguliferas of Cretaceous times. The possibility of such a comparison has naturally led the way to the view that the enigmatical Tanganyika animals may not in reality have resulted from the marine contamination of the lake, but be the survivals of a prehistoric fresh-water stock, which once upon a time existed widespread in Africa and in Europe and America also. But although there is the most remarkable similarity between the Paramellanias of Tanganyika and the fossil Pyrguliferas, it must, I take it, be confessed that the determination of a real identity between fossil and living shells of this sort is hazardous in the extreme. For example, if the singular Lymnotrochus of Tanganyika had become fossilised in ancient beds, it would almost certainly have been classed along with the true Trochi, although in reality it is nothing of the kind. On these slender grounds, of a similarity

[1] I have discussed the evidence bearing upon the relative antiquity of the lakes in a paper in the *Journal of the Royal Geographical Society* now in the press.

[2] *Proc. of U. S. Nat. Mus.*, S. 98, Washington, 1883.

[3] *Sitzungsber. d. Kais. akad. d. Wissensch.*, Bd. xc., 1, p. 57.

between ancient and living fresh-water shells, the anatomical characters even of the latter not being then known, the idea has been promulgated that the whole of the isolated group in Tanganyika has had no marine origin but represents a sort of prehistoric condition of the African lake faunas in general. There are, however, to my mind several fatal objections to such a view, first and foremost among which is the total isolation of the Tanganyika group to the lake in which it now lives. If this group were really representative of the ancient African fresh-water Cretaceous stock, more or fewer of the remaining lakes would almost certainly have contained representatives of this group, and although it has been argued that change in the salinity of the water, of which there is no proof, or some other conditions not specified may have swept the old faunas out of the remaining lakes, it is to my mind in the highest degree improbable that such causes would ever have exterminated the animals in question out of all the lakes but one. Then again the gastropods are associated with jelly-fish, animals which have always been regarded as typically marine, and to argue that in the past times jelly-fish were fresh-water organisms, because certain enigmatical shells in a lake appear to correspond with fresh-water cretaceous forms, is to my mind to make a gratuitous assumption which the facts of the case do not in the least necessitate. Lastly the Paramelanias themselves although presenting in the characters of their shells, such a marked similarity to the Pyrguliferas of the chalk, are quite as much like some of the marine jurrassic Purpurinas, and in this way the Palæontological evidence upon the subject can be made to cut both ways. Moreover, supposing the Paramelanias to be really the living representatives of the Cretaceous Pyrguliferas as some of the latter were estuarine forms, it is quite likely that they may have been existing in or about Lake Tanganyika, if this lake *was* in connection with the sea, and have lived on in the lake along with those marine animals which were able to stand a succession of later changes.

The palæontological evidence touching the origin of the

Tanganyika fauna is then in a most complicated and uncertain condition at the present time, but it is quite likely that more light will be forthcoming in this direction later on.

There is yet one more possibility concerning the origin of these forms which it is necessary to take into account. It is quite conceivable that the prolonged isolation which these animals living in Tanganyika have undergone may have allowed them to sport into forms simulating oceanic types, and this is in itself a most attractive hypothesis, but it is again met by the same fatal objection of the complete restriction of the marine fauna to the confines of Tanganyika itself. If these organisms were the result of sporting, why have not the faunas of the other great lakes exhibited the same sportive characters; why, for example, has the fauna of Nyassa retained its original fresh-water characters although there is more geological evidence for the antiquity of the fauna of this lake than in the case of that of Lake Tanganyika.

From all this it is apparent that the alternate hypotheses to marine contamination which may be made to account for the presence of certain animal forms in Lake Tanganyika seem one after another either to break down entirely or to become so uncertain as to be of little value, except as incentives to the production of evidence the combined effect of which has been to leave the theory of marine contamination as the only road towards the solution of the problems which the African lakes present.

Thus without appealing to the facts of morphology themselves, and quite apart from the affinities which the Tanganyika group really exhibit, it seems quite impossible to account in any way except through the hypothesis of direct marine contamination for the presence of certain animals in the lake. These objections are, moreover, something more than negative; in every case they afford positive evidence against the possibility of adopting any of the alternate hypotheses detailed above. The only remaining objection which can now be raised apart from the direct morphological evidence which may bear upon the theory of marine contamination is that such a view is rendered im-

probable by the geology of the district in which Tanganyika lies. There is, however, no such objection forthcoming; what is known of the geology of this district simply shows that the lake region has been enormously elevated and depressed in by no means very ancient times, and this is in exact accordance with the facts of morphology, as we shall see.

We have then, I hope, cleared the way to a direct appreciation of the facts of morphology themselves. We have seen that a greater number of the hypotheses which have been made to account for the peculiarities of the African lake faunas are inapplicable to the questions they were intended to solve, when the facts of environment and distribution have been fully weighed, and, lastly, that there is no evidence either geological or palæontological which in any way militates against the conclusions drawn purely from a study of the morphology of these forms being any way unsound. Whatever result can be got out of the study of the pure morphology and affinities of these animals is likely to be the best explanation of their existence in Tanganyika that can be obtained.

It would, however, be premature as well as beyond the limits of this article to enter into any detailed account of the anatomy of the individual members of the unique Tanganyika group; all I shall do is to indicate the general tendency of the investigation, as far as it has been at present carried.

The incapacity which certain members of the Tanganyika fauna present, of being directly associated with any other living species, is a most remarkable fact; it shows among other things that these aberrant lake forms must have had time to remain as they were until similar forms had become elsewhere extinct, and thus these animals still living in Tanganyika must be comparatively old.

The Medusa which was first sent home by Mr. Swann and examined by Günther has never been satisfactorily associated with any living form. The deep-water gastropods which I obtained in the lake are in the same predicament, but these forms offer many structural points, which

throw a considerable light on the real nature of the fauna to which both they and the jelly-fish belong. The Typhobias of Tanganyika which might at first sight have been regarded as something of the nature of a Paludina with a highly specialised shell, appear really to combine the anatomical characters of Bythinia, Natica, Littorina, and in some respects even of Strombus, while at the same time they possess two forms of Spermatozoa, exhibiting in this respect the characters both of a Paludina and a Murex. It would appear thus that the Tanganyika gastropods are not only unlike any living species, but that they seem to combine the characters of several, and this is exactly what we should expect to be the case if the Typhobias were a comparatively ancient form. Now what is true of the Typhobias is true respecting the genera composing the deep-water Tanganyika gastropods as a whole. We have in these animals organisms which retain characters that are found only in very distinct genera and families at the present day, and the only possible inference from all this seems to be that they retain the generalised character of ancient forms.

Lastly, these members of the Tanganyika Molluscan group which I have examined appear, on the whole, to possess characters combining those of several *living marine* genera and not those of any typical *fresh-water* forms.

As is well known many of the fresh-water Molluscs, like so many other fresh-water organisms, retain the characters of certain extremely ancient forms, and it is obvious from this that the forerunners of the modern fresh-water Molluscs, as for example those of the Cretaceous times, would generally be less specialised than their modern derivatives and would exhibit the remains of their ancient organisation in a more marked degree. If the Tanganyika forms were the persistent representatives of such types as these we should expect that they would present more and better marked primitive features than the fresh-water animals of the present day. The fresh-water Molluscs of Tanganyika however, combine a number of the generic and family characters belonging to a variety of living Molluscs, and

thus they are probably no older than the time of the separation of the species of Molluscs from one another, which now possess their characters separately.

On the whole then, the members of the Tanganyika Molluscan group, which I have as yet examined, appear to possess characters combining those of several living marine genera, and thus the evidence as to their origin, which can be deduced from the anatomy and characters of these animals themselves, favours the view which I formed while still on the shores of the lake, namely, that these forms have survived as the representatives of an old contamination of Lake Tanganyika with the sea, and this view is, as we have seen, not negatived nor in reality, I believe, seriously controverted by any of the objections which have hitherto been raised against it.

J. E. S. Moore.

THE POSITION OF SPONGES IN THE ANIMAL KINGDOM: A CORRECTION.

IN my article on this subject in the last number of SCIENCE PROGRESS, I quoted Sollas as having in 1888 ("*Challenger*" *Reports*, vol. xxv.) proposed the term *Parazoa* for Sponges as a group of equal systematic value with the Metazoa, and considered by him as descended independently from the Choanoflagellata. It has been brought to my notice that these ideas were put forth by Sollas four years earlier than was stated by me to be the case, namely, in 1884 (*Quart. Journ. Micr. Sci., N.S.*, xxiv., p. 603 *et seq.*). I hasten therefore to make this correction, with regrets at having overlooked the earlier memoir.

E. A. MINCHIN.

APPENDIX.

NOTICES OF BOOKS.

The Biological Problem of To-day. By Dr. Oscar Hertwig. Authorised translation by P. Chalmers Mitchell, M.A., with an Introduction by the Translator and a Glossary of the technical terms. London: William Heinemann, 1896.

Dr. Hertwig is well known for his brilliant powers, both as an original investigator and as a critical exponent of the bearings of Biological research. It is hardly necessary then to say that his book will be read with keen interest by all who are attracted by the newer results of Biological speculation.

And the value of the translation is not a little enhanced by Mr. Mitchell's extremely clear Introduction, which will enable many to appreciate the points of the argument who might otherwise be discouraged by lack of familiarity with the subject treated of.

The object of the book is to explain the issues involved in the controversies raging respectively around Evolution and Epigenesis. Dr. Hertwig adopts the latter position, and he deals some very heavy blows at the foundations which underlie the doctrines of Weismann and of others who have more or less openly declared themselves as on the side of the Evolutionists or Preformationists.

One by one the author deals with the principles regarded as essential by his opponents; he discusses the consequences of the assumption of discrete material particles as associated with the existence of as many different hereditary qualities, and he points out how impossible it is to reconcile these assumptions with the facts of regeneration and of heteromorphosis without calling in the aid of an elaborate series of accessory hypotheses.

Having finished with the destructive criticisms levelled at the germplasm theory, Dr. Hertwig devotes the second part of the volume to the outlining of his own views as to the ways and means of organisation and development. To use his own words: "I start from the conception that the ovum is an organism that multiples by division into "numerous organisms like itself. I shall explain the gradual, progressive "organisation of the whole organism as due to the influences upon each "other of those numerous elementary organisms in each stage of the "development. I cannot regard the development of any creature as a "mosaic work. I hold that all the parts develop in connection with each "other, the development of each part always being dependent upon the "development of the whole." These statements are supported by considerations drawn from organic form, from observed correlation of development, from the modifying influence exerted by the environment,

and so on. The obvious objection, that the author does not help us in really knowing why one organism develops in this way and another in that way, is unfortunately one which we are not in a position at present to remove. In this connection Hertwig stands in sharp contrast with Weismann. The latter has formulated a theory of heredity which was intended to enable us to understand the why and wherefore of all kinds and all grades of organisation. Hertwig, on the other hand, starts with the ovum which is already endowed with the potentialities of the species, and he is content to trace the influence of the various causes which may effect or modify the course of its development to the adult organism, without saying wherein the *inherent* forces exist which primarily determine the ultimate form. But he has at least pointed out most forcibly the difficulties in the way of accepting any of the explanations as yet suggested.

Lehrbuch der Oekologischen Pflanzengeographie, Eine Einführung in die Kenntnis der Pflanzenvereine. Von Dr. Eugen Warming. Deutsche ausgabe von Dr. Emil Knoblauch. Berlin: Gebrüder Borntraeger, 1896.

It has often been a subject of lamentation, in this country at any rate, that Naturalists are not so common amongst Biologists as they used to be. This is no doubt true, and it is clearly reflected in the modern type of text-book, whether Zoological or Botanical. As regards the latter science, the causes are to be sought partly in the comparative neglect into which the study of Systematic Botany has fallen. The centre of gravity has shifted, and an intimate knowledge of the minute structure, rather than of the external forms, of plants has become perhaps a little too clamorously demanded.

But signs are not lacking to indicate that the pendulum has begun to swing back again. Many of the younger botanists have already begun to perceive that study of plants as they grow is just as indispensable as is a knowledge of the structure of their vascular bundles or of their apical meristems. And on this perception it is to be hoped performance will follow. No doubt some of the disinclination to devote much attention to the systematic branch of the science has been partly due to a sort of feeling that taxonomy is rather "played out". We do not, however, by any means concede this; and certain it is that no student who desires to be in touch with the problems of the immediate future can afford to dispense with the range of knowledge which a moderate acquaintance with taxonomy alone can give.

One of the questions which in its details is gradually shaping itself more and more clearly is the important one which concerns adaptation to the environment. We know well enough that certain districts are associated with certain types of vegetation; we want further to know what it is which determines the peculiar features of these dissimilar floras. But the more closely we examine the proble[illegible]

the more intricate does it appear to be. The constituents of any one flora, be it ever so characteristic, are hardly ever uniform in their fashion of adaptation. Different plants have different ways of overcoming apparently similar difficulties. What then are the factors which determine the preponderance of this and of that type of structure?

To indicate the nature of the circumstances which are thus operative, as clearly as is at present possible, forms the main purpose of Professor Warming's book. He treats first of the physical environment, the soil, climate, illumination, state of the atmosphere, and so on, in its general relation to vegetation, and then proceeds to discuss the individual characteristics exhibited by the various kinds of floras, such as marsh, marine, alpine, xerophilous, epiphytic, and the like. He points out and emphasises the difficulties of finding a ready explanation which shall embrace the constituent features of any one flora, and further indicates how the various types may actually become mingled.

His book is full of delightful details concerning the relation of different plants with their surroundings, and if the wealth of examples he quotes only serves to frighten some of his readers into a closer acquaintance with a few additional forms of life as they live and have their being, he will not have written altogether in vain. But Dr. Warming does far more than this, and we have no hesitation in saying that his book forms one of the most important and stimulating contributions to the botanical literature of recent years. He not only indicates the rational principles which must form the basis of any intelligent study of plant distribution, but his book abounds in hints as to points which are still in need of further elucidation.

A word of caution is given against the habit of generalising too hastily as to the purpose of this or of that structure. The entire form and habitus of a plant is the result of a compromise between its different parts, and it is only by a careful comparative analysis of form and structure, coupled with a due recognition of the complicated nature of the factors which together make up the environment, that any real advance is to be hoped for.

Dictionnaire de Physiologie. Par Charles Richet, avec le Collaboration de MM. P. Langlois et L. Lapicque. Tome Premier A—B, 1er and 2e Fascicules, pp. 672, 1895. Paris: Ancienne Librairie, Germer Baillière & Cie.; Félix Alcan, Éditeur.

The advance of a science may be estimated by the growth of its literature—the method of its growth by the manner in which that literature is arranged.

In the case of every branch of knowledge it is desirable to possess, in addition to the text-books, hand-books and monographs which are essential to a proper understanding of the subject—some storehouse in which may be found a summarised account of all that is known upon any particular point. It is, moreover, desirable that in such a store-

house the contents be arranged so as to be readily accessible to any inquirer.

Perhaps the most convenient arrangement for such a general collection is that afforded by a series of articles alphabetically arranged, by, in fact, a dictionary of the science under consideration. That such an arrangement has been a favourite one may be gathered from a glance at some of the more important branches of natural science. Thus we have dictionaries of Chemistry, of Botany, of Electricity, and others, whilst there have lately appeared several important treatises on Medicine in which a similar arrangement has been followed.

For such an arrangement to be satisfactory, however, several things are necessary. In the first place the special branch of knowledge concerned must have been in existence, and separated from its sister sciences, for a length of time sufficient to provide an array of well-established facts large enough to justify the selection of an ambitious mode of treatment; and in the second place each article must be written by a specialist in that particular branch in order that a thorough treatment may be ensured. At the same time the opinions held by the writer must not be put too prominently forward.

Up to a few years ago various causes had contributed to render undesirable any attempt so to group the main facts of physiology. Chief amongst such causes may be mentioned the youth of the science as distinct from Anatomy, Chemistry, Histology and Medicine, and the small advance that had been made in several important branches. At the present time, however, most of these objections have disappeared, and the science of physiology has made such rapid advances and become so separated off from its sister sciences, whilst at the same time the mass of facts has become so vast, that some tabular arrangement seems almost a necessity.

It is, therefore, with great pleasure that we welcome the first part of the present work. A Dictionary of Physiology, by Charles Richet, assisted by MM. P. Langlois and L. Lapicque, together with a large number of other prominent physiologists, ought to, and, judging from the sample before us, we have every confidence that it will, furnish such a complete statement of the main facts of physiology as will materially aid the future development of the science.

At present we have only the first two parts of the first volume before us, and as the whole work is to consist of five volumes this is but a small instalment. Nevertheless we have here articles from which we are able to form a fair idea of the character and scope of the whole work. Thus we have an article of thirty-seven pages on Accommodation by Wertheimer, which gives a good account of what is known on the subject. Commencing with a succinct definition of the subject and proofs of its importance, the author proceeds to set forth the opinions which have been held as to the mechanism of accommodation.

The *rôle* of the crystalline lens and the cause of its change of form is described at length, and tables are given of the data of dioptric

measurements obtained by various observers (Helmholtz, Knapp, and others). The amplitude of accommodation and the rapidity with which it is effected are treated somewhat fully, and the author concludes with a description of the mechanism of accommodation through the animal series (mammals, birds, reptiles, fishes, batrachians and invertebrata).

Acromegaly is treated by Paul Blocq, Actinomycosis by F. Heim, Visual Acuity by Nuel, and Addison's Disease by P. Langlois. From this it will be obvious that the work will be of interest not to physiologists alone, but also to physicians and medical men generally. Of the advantage of physiological facts being brought to the notice of the medical profession there can be no doubt when we have before us such an instance as the recent advance in the treatment of myxœdema by the extract of thyroid gland. It is to be expected that similar results will follow the application of scientific experiments in the case of diseases at present regarded as but little amenable to treatment.

A valuable article on the Albuminoids by J. E. Abelous extends to thirty pages, and a copious bibliography is added.

Gabriel Pouchet writes an article of seventeen pages on Alkaloids, whilst more than thirty pages are devoted by Heim to the physiology of the algæ.

A very interesting article on Aliments, extending to nearly a hundred pages, is contributed by L. Lapicque and Charles Richet, whilst a short but useful paper on Aluminium is written by A. Chassevant.

One of the most important papers in the portion of the work before us is the very able historical and critical essay on Anæsthesia and Anæsthetics, by Charles Richet, in which after a historical introduction we find a discussion on general and local anæsthesia and the various agents which have been used for producing these. We are in agreement with the author in most of his statements. We note that he supports the conclusions arrived at by the Hyderabad Chloroform Commission, and believes that the heart almost invariably continues to beat—and the patient is therefore recoverable, provided proper means be adopted—for some minutes after natural respiration has ceased. We note also with satisfaction that he emphasises the facts established by Paul Bert and his pupils of the importance of administering the anæsthetic in feeble, progressive doses, rather than in strong doses without admixture with sufficient air. With his criticism of the various agents employed, however, especially with his statement that mixed anæsthetics have been given up, we are unable to agree, for the A.C.E. mixture is still largely used in some London hospitals. After all, however, this is a matter of custom more than anything else, for whilst at Edinburgh chloroform is almost always the agent selected, in London ether is far more frequently used. Some of the other articles contained in the first two parts of the first volume are:—

Antisepsis and Asepsis, -	By E. Trouessart.
Antitoxins, - - - -	,, Charrin.
Aphasia, - - - -	,, E. Lahousse.
Arachnids, - - - -	,, F. Plateau.

The short paper on Anthropology by Louis Lapicque shows how little has at present been accomplished in the way of a comparative study of the physiology of different races of men.

The book as a whole strikes us as likely to be useful, and provided the rest of the articles are up to the standard of those at present published, physiologists will be provided with a reliable and conveniently arranged statement of the more important facts of their science as it exists to-day.

APPENDIX.

NOTICES OF BOOKS.

The Diseases of Children's Teeth. By R. Denison Pedley, M.R.C.S., L.D.S. Eng., F.R.C.S. Edin. London: J. P. Segg & Co.

When the medical student, having successfully passed his last examination, settles down in the country to the practice of his calling there are many things which he finds he is imperfectly acquainted with. Even the industrious ones who have conscientiously devoted their attention to the lecturer's words and have read steadily for many months begin to realise at last that medicine is an immense subject, which it takes many years to master.

More especially is the deficiency felt in the case of those subjects dealt with in the Special Departments, both because these are considered of minor importance by many and also because in some hospitals the opportunities of work afforded by these Departments are few.

But however the deficiency of knowledge comes about, it is equally deplorable, and the newly fledged practitioner can only turn for help to his perhaps scantily furnished library. Woe to him if his choice of books has not been wisely made! Books bearing upon the special diseases of throat, ear, skin and eye will probably be conspicuous by their absence; and indeed it is scarcely desirable that the general practitioner should undertake any but the most trivial operations in connection with these, always supposing that he is within reach of a good hospital.

In the case of the teeth, however, it is different. With a comparatively small amount of knowledge, much can be done to preserve—at any rate for a time—teeth which without attention would infallibly be lost. Of still more importance is it that proper attention should be devoted to the teeth of children, in whose case lack of proper care may, and very probably will, result not merely in deformity of the mouth, more or less serious, but also in the premature loss of members of the permanent dentition.

A book in which the student and the young practitioner can find set before them in a simple and yet thorough manner the more important points to be attended to in the care of the teeth, and the

best means of dealing with mischief which has already occurred should such necessity arise, will be valuable in proportion as its explanations are lucid and its directions easily followed. In the book given us by Mr. Denison Pedley we have a perfectly clear explanation of the changes mostly to be feared in the teeth of children (and to a great extent in the teeth of adults also), together with instructions as to the best means to be taken in order to avoid subsequent mischief to the permanent dentition.

That such attention to children's teeth is desirable is plainly indicated by the figures given in the chapter on Oral Hygiene, where we learn that of 3800 children examined there were only 828 in which neither filling nor extraction was required. Of temporary teeth there were, in 1460 children, 3187 which could have been saved by proper attention, whilst the number requiring extraction was 2491. There were 3511 permanent teeth which required filling, and 1506 were so carious as to require extraction.

The children examined belonged to the poorer classes, and the boys are either taught a trade or enter the army or navy. The girls are trained for domestic service.

During 1890, 55,673 recruits were examined and 21,712 were considered unfit for service. Of these, 506 were rejected on account of the "loss and decay of many teeth".

The troubles of a domestic servant suffering from neglected teeth need little imagination to picture. Besides the pain of the decaying teeth themselves, we have to consider the troubles of the digestive and other systems, consequent upon the presence of carious teeth in the mouth.

We are glad to notice that Mr. Pedley continually draws attention to the effects of carious teeth on the nervous and other systems, as we have long been of opinion that much of the pain complained of by patients is due to defective teeth. Indeed, the improvement produced in the general health by the removal of the diseased structures is often very great, and this may even be the case when no pain in the mouth has been experienced.

Of the advantage likely to be gained by the removal of carious teeth in the case of children suffering from chorea, the following is a good example:—

> H. B., aged eight years. Chorea of seven weeks' standing. She did not improve at all during the first two weeks in the hospital. A painful stump was removed. In four days all movements ceased and did not recur.

This is only one of numerous examples given.

Mr. Pedley also gives examples showing the good resulting from a similar procedure in cases of diarrhœa caused apparently by the reflex nervous irritation of decayed teeth, and attempts to show that other diseases due to the absorption of septic material may be caused by infection taking place through carious teeth.

We see no reason to suppose that such infection is in any way impossible.

In the chapter on Treatment the main points to be observed and the best means of dealing with the more common forms of disease are set forth with admirable clearness. The instructions given are sufficiently full to enable the practitioner always to relieve pain and in almost all cases to do sufficient in the way of stopping the decayed teeth to ensure their preservation until such time as a dental surgeon can be consulted. Stress is laid upon the importance of recognising the first permanent molar from its neighbouring temporary teeth, as, whilst the latter have only to serve their purpose till about the twelfth year, the former has to last the child through life.

In the matter of extraction of teeth, directions are given for the effectual removal of the various teeth from the upper and lower jaw, and those cases are indicated in which this drastic treatment is absolutely necessary.

The book ends with a short article on Tartar and its removal.

We wish cordially to recommend this work of 260 pages, not merely on account of the clear and concise manner in which it is written, but also because of the immense importance to the comfort and well-being of the community of the matters of which it treats. Indeed, we regard it as a book which no general practitioner can afford to be without.

A New Course of Experimental Chemistry. By John Castell-Evens, F.I.C. Thomas Murby. Price 2s. 6d.

This book consists of three parts. Part I. (pp. 1 to 96) is a mixture of arithmetical problems and elementary chemical experiments, the experimental work only occupying thirty-three pages. So curiously is this mixture compounded, that on casually turning over the pages one might almost fancy that by mistake a few stray leaves of some elementary practical chemistry had got accidentally bound up among the sheets of a book of arithmetical problems. There appears to be little connection between the experiments and the problems; thus, pages 17 to 24 contain physical problems on specific gravity, specific heat, latent heat, heat and work. These eight pages are followed by three pages of practical matter, consisting of a few experiments designed to show the composition and properties of water. Then come eight more pages of physical problems, including exercises on the dynamical theory of gases, in the middle of which there is sandwiched a single page containing four more experiments on water.

Of the problems themselves little need be said; most of these will be useful exercises for the student. Some, however, are a little ambiguous; *e.g.*: "Four grains of iridium at 1400° C. are put into 100 grains of mercury at 10°; what will be the result?" (p. 64).

Others require correction; thus on page 9: "Give on the three scales the absolute temperature corresponding to the following 'critical temperatures' of various vapours and gases". Of the eight gases named, the critical temperatures of six are incorrectly stated. Although this fact is of no consequence from a purely arithmetical point of view, it is obviously desirable from other considerations that the temperatures given should be the true ones.

The directions which the author gives for carrying out the various experiments suffer very much from over-condensation; thus, a student to whom it is necessary to say "cut a piece of clean *solid* phosphorus" (p. 11), or "fill six small cylinders with the evolved gas over *clear tap* water" (p. 38) (the italics are ours), would be at some loss to know how to set about the following experiment: "Put about 10 grains of fairly finely divided copper in a hard-glass tube, weigh it, and having raised it to a fair red heat pass about 5 litres of air repeatedly over it. When no further change occurs in the volume of the air stop the experiment and allow the tube to cool, then weigh it" (p. 13).

The book contains no illustrations, nevertheless we find such directions as the following: "Through the tube A B (App. 76, *a*) pass a current . . . close the taps E and F, and into the limb B pour some mercury," etc., etc. (p. 61). One seeks in vain for any explanation, but what "App. 76, *a*" refers to does not transpire.

The arrangement of the subject matter is somewhat lacking in method; thus, the section headed "Studies on Combustion" is made to include the following:—

Preparation of sulphur dioxide from copper and sulphuric acid.
Conversion of sulphurous into sulphuric acid by nitric acid.
Formation of sulphur trioxide.
Experiment to illustrate the manufacture of oil of vitriol.
Preparation of nitric acid from nitre and oil of vitriol.
Preparation of ammonia from ammonium chloride and lime.
Misleading definitions of *acids* and *alkalis*.
Experiments on neutralisation.
Experiments showing heat evolved on the solution of certain substances in water.

Part II. (about 120 pages) is called "Introduction to Chemical Analysis". In it the author attempts to teach qualitative and quantitative analysis simultaneously. Without discussing the doubtful wisdom of this plan, it may be enough to point out that by the time a student has reached the stage of testing for sodium, potassium, and ammonium compounds, he will, if he has followed the course laid down in the book, have made some hundreds of quantitative estimations. The sections devoted to groups I. and II.*a* (silver, lead, mercury, bismuth, copper, and cadmium) include alone upwards of fifty quantitative determinations. Indeed, the author seems to have tried to squeeze the whole science of chemical analysis into the compass of his little book; thus, in the five and a half pages given to the reactions of the negative (acid)

radicals of group I., quantitative estimations of the following out-of-the-way elements are included—*tungsten*, *vanadium*, *selenium*, *tellurium*, *molybdenum*, *titanium*, and *uranium*. The condensation necessary in order to pack so much matter into so little space, reduces the descriptions of methods to the most sketchy outlines, and the student would find it impossible to carry out the experiments without constantly applying elsewhere for instruction. This, however, is apparently what the author aims at, for at the beginning of Part III. (a useful Appendix, containing brief directions for making certain inorganic preparations), he says: "Let it ever be borne in mind that the great object of every experiment is not the mere attaining of the actual result of such experiment, but to give the student opportunities for reasoning and thinking, and the teacher opportunities for helping". A class of students working through this book would most unquestionably afford their teacher ample opportunity for helping; whether the other desirable end would be accomplished is not quite so certain.

Kirke's Hand-book of Physiology. By W. D. Halliburton, M.D., F.R.S. Fourteenth Edition. London: John Murray, Albemarle Street, 1896.

No better means could be adopted of gathering a general idea of the progress and evolution of Physiology, not only as taught in our medical schools but generally as a Science, than to arrange in chronological order the fourteen editions which have done honour to the name of Kirke. Perhaps the most striking evidence that could be derived from such a comparative examination would be the gradual emancipation of Physiology from the influence of pure anatomy and surgery. In the days of the early editions of Kirke it was the anatomists and surgeons who taught Physiology, and it was anatomists and surgeons who edited Kirke, as witness the names of James Paget, William Savory, and Morrant Baker, all of whom were associated in turn with the editorship, and all of whom were distinguished Fellows of the College of Surgeons. Professor Halliburton, the present editor, is the first, in a long series, who is in no way associated with the teaching or practice of surgery, moreover he is a fellow of the sister college, namely, the College of Physicians, a Physiologist of European reputation and a Fellow of the Royal Society. The present edition severs the long historical association between Kirke's Physiology and St. Bartholomew's Hospital. For fifty years this great hand-book has been edited from the latter school of medicine; and by a strange irony of fate almost the very day that gave birth to the new edition also witnessed the death of Morrant Baker, who had for so many years been intimately connected with its destinies. The new edition is entirely re-written, and, excepting in external appearance, bears little resemblance to its precursors. The previous editions had all followed closely on the lines of the ones that immediately preceded them, and in this connection it is interesting to note

that Morrant Baker in his first preface remarked that in editing Kirke only two courses were open: either to entirely re-write the work, or else to follow closely on the lines of the original author, adding such matter as the progress in Physiology made imperative. Morrant Baker adopted the latter course throughout his seven editions. Dr. Halliburton has accepted the other alternative; and the magnificent text-book which he has elaborated proves that his decision was a wise one, and that his confidence in himself was fully justifiable. Although Professor Halliburton is perhaps best known as an investigator of the chemical side of Physiology, it is impossible to detect in the new edition any undue preponderance of matter dealing with this side of the Science; indeed, if one were to pick out any one section as being better than the others, it would perhaps be that which treats of the structure and functions of the nervous system. In this, all the latest work of Ramon y Cajal, Mott, Golgi, Gowers, and many others, is ably summarised, without being overburdened with excess of technical detail, and a most valuable addition is a projective scheme of the cells and fibres of the brain, spinal cord, and peripheral end organs, constructed for the work by the author, with the assistance of Dr. Mott. This scheme presents most adequately the state of our present knowledge on the subject, and is founded on sound physiological data, and contrasts strikingly with many of the hypothetical schemata of a few years ago. Another valuable addition is that excellent diagram of Ramon y Cajal of the expansion of the auditory nerve in the cochlea. Altogether, there are something like 100 more illustrations in this edition than in the one that preceded it, and many of them greatly enhance the value of the hand-book from the teacher's as well as the student's point of view. As for the text, where all is excellent, it is unnecessary to particularise, and it is not too much to say, that if "Kirke" has lost anything in the abandonment of old tradition, it will certainly gain in prestige in that wider sphere of physiological thought and teaching over which Professor Halliburton exercises an undoubted influence.

A Popular Exposition of New Zoology. By K. Mitsukuri. Tōkyō: 1895.

Under the above title Professor Mitsukuri of the Imperial University of Tōkyō, Japan, has lately issued a popular work dealing with the broad principles of the study of animal life. The book is fully illustrated by woodcuts, which are simple and well chosen. After an Introduction, in which a well-selected list of General Treatises is given, there follow seven chapters dealing in order with "The Natural World," "The World of the Living," "The Number of Living Things," "The Kinds of Animals" (three chapters) and "The Fundamental Principles of Classification". These are succeeded by seven other chapters, devoted in succession to the Anatomy, Histology, Reproduction, Development, Growth, Distribution, and Colours of Animals; and the whole is

summed up in a "Conclusions" chapter, which is followed by a couple of Appendices, in which some good advice is given to teachers and those who would pursue the study of Natural History alone. Despite the rapidity of scientific advancement and assimilation of Western ideas in the far East, the study of Zoology is still largely regarded as a more or less harmless pursuit, interesting it may be, but unworthy the serious attention of the intellectual man. Professor Mitsukuri's book is in reality a plea for the Science of Zoology. It is charmingly written and, for an elementary treatise in an Eastern tongue, remarkably free from dogmatism. It has been the outcome of a dozen years' careful compilation and forethought, and is in every way worthy the important work which its author and his fellow zoologists have accomplished, by which none have more greatly benefited than we at home.

Mechanics. By Linnæus Cumming, M.A. London: Rivington, Percival & Co., 1896. 247 and viii. pp. Price 3s.

This is a small elementary text-book designed for the use of school-boys, and embodies the results of the author's experience in teaching a class in experimental mechanics at Rugby School. The order in which the subjects are treated is Statics, Dynamics, and Hydrostatics. Although there is no doubt that to the average beginner statics is much easier than dynamics, there are many objections to the order adopted by the author. Not the least of these lies in the fact that all through the statics the forces are measured in pounds. When, however, dynamics is reached a new unit of force, the poundal, is introduced, and suddenly what has up to now been called a force of x pounds is called a force of x pounds weight. The author in this connection does not explain how in the gravitational system the fundamental units are those of length, mass, and *force*, and that the unit of mass in this system is really g lbs. There are one or two slips we have come across; thus on page 138 it is stated that every physical quantity can be expressed in terms of the units of length, mass, and time, when it is now generally considered that temperature, specific inductive capacity and permeability form subsidiary fundamental units, at any rate in the present state of our knowledge. Again, a water-wheel is said always to work by the abstraction of the kinetic energy of the water, while in England the "overshot" wheel is quite common. The style of the book is on the whole clear, and the experimental illustrations easy of performance and carefully described, so that the work will probably be found useful for teaching beginners.

An Introduction to Structural Botany. Part II.—*Flowerless Plants.* By Dukinfield Henry Scott, M.A., F.R.S., etc. London: Adam & Charles Black, 1896.

It is with much pleasure that we welcome the appearance of the second part of Dr. Scott's *Structural Botany*. The author deals with

the Cryptogams in this volume, and it forms indeed an admirable introduction to a study of these plants.

The treatment adopted is methodical, and the essential points of contrast and comparison between the different groups are well brought out. It is satisfactory to notice that that highly specialised form, *Marchantia*, which has usually had to do duty as the representative of the Hepaticæ has here given way to a more generalised type as seen in *Pellia*. We cannot, however, suppress a feeling of regret that space did not admit of a little more recognition being accorded to the foliose of Liverworts. Not only are they exceedingly numerous as regards species, but they are also very rich in interesting forms.

The Algæ and Fungi strike us as being very well done, and we are glad to see that *Ulothrix* occupies the prominent position to which its own intrinsic importance and the ease with which it can be obtained justly entitle it.

The clear summaries and the numerous illustrations increase the value of one of the very best elementary text-books of Botany which have appeared for many a year.

Worms, Rotifers and Polyzoa. Being Vol. II. of the Cambridge Natural History. Edited by S. F. Harmer, M.A., and A. E. Shipley, M.A. "Flatworms and Mesozoa," by F. W. Gamble, M.Sc. "Nemertines," by Miss L. Sheldon. "Thread Worms and Sagitta," by A. E. Shipley, M.A. "Rotifers," by Professor Marcus Hartog, M.A. "Polychœt Worms," by W. B. Benham, D.Sc., Hon. M.A. Oxon. "Earthworms and Leeches," by F. E. Beddard, M.A., F.R.S. "Gephyrea and Phoronis," by A. E. Shipley, M.A. "Polyzoa," by S. F. Harmer, M.A. London: Macmillan & Co., Limited, 1896.

The death knell of the "Class Vermes" has long been rung, and under orderly arrangement, consequent on extended and detailed investigation, the vast accumulation of animals which in the last generation were relegated to that assemblage have been resolved into a series of well-defined groups which form the subject matter of the greater portion of this volume. In effecting this re-arrangement none have been more active in our own country than Messrs. Beddard, Benham, Gamble, Harmer, and Shipley—each a foremost worker in his own department; and when it became known that these gentlemen were to co-operate with Miss Sheldon, a recognised investigator of allied forms of life, and with Professor Hartog, in the authorship of the volume now before the world, expectation ran high, in the hope of a work which would do credit to English zoologists, and would be found to contain something beyond the hum-drum colourless take-all-and-do-as-you-like-with-it recapitulation of ill-assorted details, familiar in a certain class of continental text-books. It was to be expected that that judicious selection of facts which is the highest function of a scientific author would be brought

into play. The time has passed at which one writer can be expected to produce unaided a well-balanced handbook on all branches of Zoology, and the co-operation of a number of authors for the production of the present treatise alone gives it a claim upon our attention.

Let it be said at once that as concerning the "Vermes" our fullest anticipations have been realised, that the chapters on Rotifera by Professor M. Hartog and on Polyzoa by Mr. Harmer are equal in merit to the rest, and that the whole volume is a worthy successor to its immediate predecessor on the *Insecta*—than which no better text-book exists in the English language.

The volume opens with a scheme of the classification adopted, carefully arranged in tabular form with full-page references to the text, and then follows Mr. Gamble's section on the Platyhelminthes and Mesozoa. The ninety-six pages of this teem with interest and contain many very new and careful observations; and not the least noteworthy feature of it is the introduction of new figures of rare merit. Figures such as Nos. 5 and 17 both in execution and design are of an entirely new order, and they reflect the highest credit on author, publisher, and all concerned. The inclusion of Frenzel's *Salinella* and Schulze's *Trichoplax* among the Mesozoa is perhaps premature, and it were better to have dealt separately with such forms pending confirmation and fuller investigation.

The Nemertinea, Nemathelminthes, and Chætognatha, are treated in a thoroughly sound elementary manner, although we could have wished for more illustrations of the former.

It is on turning to the Rotifera, Chætopoda, and Hirudinea, that we meet with the greatest novelties and newest treatment. For the first time in his own language, the English student is placed in possession of an adequate account of the structural limitations of the Oligochæta, Hirudinea, and Rotifera, free of cumbersome detail and embodying those salient facts of structural similarity and dissimilarity upon which alone the inter-relationships of the groups and their position in the scale of animal life can be at all adequately determined. More especially be it noted that the existence among Oligochætes of female gonads and ducts in a state of organic continuity, and of male pores in front of female, that the development and metamorphosis of the cœlom among the Hirudinea, together with other features which unite those animals to the Chætopoda and necessitate the rejection of the time-honoured idea that Leeches arose directly from Platyhelminthes, are luminously set forth by Mr. Beddard. And he gives the student an up-to-date insight into facts of structure and development which in course of prolonged observation have led him and others to the conclusion fast gaining ground that the metamerically recurrent type of nephridium is the more primitive, and that between the diffuse excretory apparatus of certain Chætopoda and Hirudinea and the Platyhelminthes there may be no close relationship—the latter being perhaps a system *sui generis*, in which the tubes seek out the primary centres of destructive

metabolism, much as do the tracheæ of Insects. More might perhaps have been said concerning the limits of modification and development of the chætiparous system, and it is not a little remarkable that Mr. Beddard should have omitted reference to the larval sensory apparatus of *Acanthodrilus* which he himself discovered.

Dr. Benham's chapter opens with a sufficiently detailed and guarded account of *Dinophilus*, *Polygordius*, and *Pontodrilus*, classified together as the Archiannelida. He then passes on to the Polychæta, and gives us a succinct account of their structure, development, and life history, with ample allusion to habits—especially as related to peculiar morphological adaptations. We note with much satisfaction the incorporation of the very careful and praiseworthy observations of Watson upon tube-formation, of the like of which we cannot have too many in these days of reckless generalisation upon wholly morphological characters. Asexual reproduction is adequately treated, the wonderful *Syllis ramosa* and *Eunice tibiana* find appropriate places in the text, and, as with other sections of the work, coloration, luminosity, and regeneration, are in due course considered. Distribution and palæontology come in for due notice, not altogether with the novelty which characterises the work of other contributors to the volume, however, for the paragraph on the "palæontology" of the Nemertines is worthy that so notorious on Snakes in Iceland. Better and more numerous illustrations appear to us desirable for this portion of the work, especially as delineating the "Trochosphere" larva. That given (fig. 145) is exceedingly poor, and even the familiar "Lovén's larva" series of earlier text-book writers would be welcome.

Concerning phosphorescence, we are pleased to note that the author of the chapter on the Oligochæta omits all reference to the paper by Secchi, cited in his "Monograph of the order Oligochæta" to which he frequently alludes, for it in no way deals with Earthworms.

For the two concluding chapters by the Editors we have nought but praise. Their well-known researches during the last decade upon groups of animals dealt with in this volume led us to anticipate success, and they have not disappointed us. Novelties like the discovery of the Nemathelminthes Arhynchidæ, and careful observations like those upon the processes of budding and reproduction among the Polyzoa, are now seen to have been related to a laborious piece of work on the student's behalf, for which it is difficult to adequately return thanks. We are especially grateful to Mr. Harmer, whose pages of the book are a fitting corollary of his well-known monographs, which have always appeared to us of equally high literary and scientific merit.

Mr. Harmer deals by preference with the British Polyzoa, and gives a valuable key to their identification; but we do not consider this sufficient justification for omission of all mention of the arenaceous Polyzoon *Cryptozoon* discovered by Bracebridge Wilson and described by Dendy from the Australian seas, and of genera so remarkable as

Kinekoskias and *Ascorhiza*. We could have wished also a fuller treatment of the lophophore and epistoma, especially as concerning their probable non-homology throughout the Brachiopoda and Polyzoa, and at least a mention of spicule formation.

The use of the "etc." is in a scientific work indefensible. The term "trochosphere" might have been advantageously replaced by "trochophore" throughout, and mention might have been made of the important observations of Goto upon *Diplozoon*, and of Hill upon the development of the cysticercoid in *Polycercus* and *Monocercus*, and of the scolex in *Synbothrium*. We note with pleasure the recognition of Goto's masterly monograph upon the Trematoda monogenea of Japan—foremost among the productions of the East in systematic Zoology, and could have wished full reference to Martin Woodward's observations upon the numerical variation of the ovaries in the common Earthworm. Wilson's alleged discovery of the opening of the head-kidney of the larval *Hydroides* into the proctodæum should have been recorded, together with a number of other discoveries by contemporary investigators, recapitulation of which is in these pages impossible.

Zoology, like most things, is now suffering from the effects of the scramble of life which has overtaken all men; and it is interesting, though accordingly not surprising, to note that during the publication of this work the mysterious *Trochosphæra* has turned up in the Havana river, that living Land Nemertines have been found in New Zealand, and that Tower has unravelled the detailed structure of the Cestode nervous system. And we find ourselves unable to decide whether the recent discovery of the entoparasitic *Myzostoma artemia* was or was not announced in time for incorporation in the volume. Burger's revolutionary classification of the Nemertines has just come in time for incorporation, and we hail with delight that of Kowalevsky's work on the wonderful germs *Acanthobdella*.

There are many parts of the book which might be extended, but it must first be tried in its present form. *Phronis* is rightly placed approximate to the Polyzoa, but *Cephalodiscus* and *Rhabdopleura* stand over for treatment with *Balanoglossus* and the vertebrata. We could have wished it otherwise, and we await with interest in this connection the volumes on the Echinodermata and Chordata. Professor Hartog rejects the time-honoured comparison of the Rotifera with the Trochophore, with its familiar refrain, in preference for a Pilidium larva association, which seems to us even less justified. The biporal stage in the development of *Amphioxus* suggestive of the remarkable *Salinella* appears to be passed through by some Echinoderms; all the more reason therefore that the Mesozoa should have been dealt with independently. Indeed, with regard to the inter-relationships between the present volume and its predecessors, we would have preferred the treatment of the Polyzoa, Brachiopoda, Rotifera, and Gephyrea, with *Phoronis*, in one volume, and the reservation of independent volumes for the rest of the "Vermes" and the Mollusca.

Original illustrations such as figs. 5 and 17 and copies such as figs. 12, 18 and 234, are not to be beaten; but we would have more of them. Paper and printing leave nothing to be desired; and, as for style, the portions of the work dealing with the life-history of the Tematoda and the fissiparous process in the Polychæta read like a romance. The Editors have this time kept their forces well in hand, and with co-authors and publishers have executed their work in a manner worthy a new departure by a great nation.

APPENDIX.

NOTICES OF BOOKS.

Alternating Currents and Alternating Current Machinery. By Dugald C. Jackson and John Price Jackson. Pp. xvii and 729. New York: The Macmillan Co., 1896.

This is the second volume of a text-book on electromagnetism and the construction of dynamos, and deals, as the title shows, with alternating currents and alternating current dynamos and motors. Unlike many books on kindred subjects that have been published it is neither a catalogue of commercial machines with the figures taken from makers lists, nor is it a text-book on pure mathematics with nothing but its title to connect it with actual practice. In the fourth chapter which deals with graphical and analytical methods of solving problems in alternating current circuits the authors have, perhaps, allowed the mathematical part of the subject too much play and have gone into cases which are hardly likely to ever occur in practice. On the whole, however, we strongly recommend the book to the attention of those who, being engaged in practical electricity, are not frightened because on opening a book they see the integration sign.

The Elements of Physics. By Edward L. Nichols and William S. Franklin. Vol. ii. Electricity and Magnetism, pp. 261. New York: The Macmillan Co., 1896.

This volume forms the second of a complete course on Physics intended as a college text-book. Although there are 227 cuts in the 261 pages and the type is large the authors attempt to touch upon most of the points in Electricity and Magnetism. The result is that as a note-book for refreshing the memory the work is excellent, but we sincerely pity the student who attempts to tackle the pages without having previously acquired a fairly extensive knowledge of these subjects. A very serious objection to the book from the student's point of view is that the references are most of them to § 000. This careless omission occurs again and again not only when the cross reference is to the third volume, but even when it is to another part of the volume under notice, and also when referring to original papers in Scientific Journals the page is given as 000.

How to Study Wild Flowers. By the Rev. George Henslow, M.A., F.L.S., etc. With 57 Illustrations. Religious Tract Society. 1896.

The writer of this little volume desires to impress on the teachers of Botany in schools the necessity of accuracy on the part of the pupils.

Admirable advice; but why did the author himself refrain from setting the first example? We do not mean to imply that the book is entirely a bad one, far from it; some good points it undoubtedly possesses, and it may even prove useful. But does Mr. Henslow really think that gymnosperms, as illustrated by Coniferæ, are to be regarded as Dicotyledons? And why does he confound Endosperm with the very different body Perisperm? These examples by no means exhaust the errors which a glance through the pages of his book has sufficed to discover; but the mention of them is enough to prove that, although the general line adopted may be reasonably commended, anybody who relies exclusively on its author's guidance (in spite of his "experience as an examiner for the past forty years") will subsequently find out that he will have some things to unlearn.

Essays. By George John Romanes, M.A., LL.D., F.R.S., etc. Edited by C. Ll. Morgan. London, New York and Bombay: Longmans, Green and Co., 1897.

Many people will welcome this volume of essays by the late Mr. Romanes which Mr. Lloyd Morgan has collected from various reviews and periodicals.

The essays themselves hardly perhaps demand criticism; they were written originally as contributions to current literature, and some of them date as far as sixteen years back. But they possess more than the mere ephemeral importance which often attaches to magazine articles; each one of them is a serious and honest attempt to deal philosophically with questions which not only interested people in the past but which still continue to attract the attention of educated persons. And it is no small tribute to their intrinsic excellence to be able to say that they still retain in a remarkable degree a decided freshness and appropriateness to present conditions of thought.

In brief the collection is one which not only the friends and admirers of Mr. Romanes (and they are many) will peruse with renewed pleasure, not indeed unmixed with sadness, but one which the general reader will be glad to see rescued from the comparative oblivion of bound volumes of Reviews.

Diseases of Plants Induced by Cryptogamic Parasites. By Dr. Karl Freiherr von Tubeuf. English edition by William G. Smith, B.Sc., Ph.D. Three hundred and thirty Illustrations. London, New York and Bombay: Longmans, Green and Co., 1897.

The study of Plant-pathology is one which has been almost neglected in this country, and yet from a practical point of view it undoubtedly ought to be regarded as of great importance. The comparative oblivion under which it is suffered to remain is partly due to the somewhat academic fashion in which Botany is chiefly pursued in England, and this again is brought about by the useless but none the

less burdensome pressure of the examination system which gives scant encouragement to wander into branches of study requiring the expenditure of much time in the field—time which could be so far more economically and profitably (!) employed in getting up facts from books and lecture notes, or in poring over sections in a laboratory.

And indeed there is little enough to stimulate any one in this country to undertake this kind of work. We are a "practical" people, and we show our appreciation of the value of scientific help by allowing our forests, in many cases, to get on as best they can under a mere rule of thumb management. This course however has at least the merit of providing those who care for such things with museums of mismanagement on a not inconsiderable scale.

But in Germany they conduct things better. There forestry is a serious pursuit, and the scientific assistance rendered to the forestry departments by a properly trained staff of pathologists and botanists working in suitably equipped institutions is appreciated as a matter of the highest importance to an industry which is conducted on a purely commercial—and remunerative—basis. There is no reason why silviculture should not pay equally well in this country if its rudiments were but understood.

Much of the land now lying idle as the result of conditions of agricultural depression which are not likely to be materially ameliorated, might profitably be afforested, but it is of little use to go in for new planting in the ignorant and blindly stupid fashion which is only too prevalent in matters relating to landed affairs in England. "Where the carcase is, there will the eagles be gathered together," and where a forest is there will always be diseases of various kinds to combat. A successful issue to the struggle can only depend on the intelligent application of remedial measures based on an accurate knowledge of the structure and constitution both of the trees and their enemies.

The translation of von Tubeuf's book will, at any rate, make clear to those persons who are disposed to think at all on these matters, that the subject of Plant-disease is a large one. The author expounds in the introduction the general nature of fungal parasites and saprophytes, and their relations to their living or dead hosts as the case may be. It is tolerably certain that his views on some of these matters will not secure very general acceptance; this, however, does not affect the value of the second and far larger part of the volume in which he treats of the individual fungi, indicating their method of attack, and the kind of damage they effect. Remedial measures are also often suggested for particular cases.

The work is illustrated by a large number of figures and photographs. The latter, however, can hardly be praised; indeed, they are often so badly reproduced as to be almost valueless. But in spite of this, we can confidently say that every one who is at all interested in these questions ought to have the book in his library. Many persons who would find it difficult or tedious to read the original will be grate-

ful to Dr. Smith for his translation, which, if rather laboured at times, appears on the whole to be well and accurately done.

Smaller Atlas of Illustrations of Clinical Surgery. By Jonathan Hutchinson, LL.D., F.R.S. London: West, Newman and Co.

Mr. Jonathan Hutchinson's original *Illustrations of Clinical Surgery* in folio size, in two volumes and in price almost prohibitive has been more frequently a source of envy to those who could not afford to buy it than a joy to the envied few who could afford to do so. By the publication of the *Smaller Atlas*, a work dealing with similar but not the same subjects, by reason of its modest price, the possibility of possession is extended to the many. As in the case of the larger Atlas, the series of illustrations is conducted on no fixed and systematic lines; it is merely a collection of pathological curiosities, and, as Mr. Hutchinson remarks, he has selected what interested himself in the hope that it would interest others. Those who have attended his clinical *séances* and those who have examined the specimens in his clinical museum and are thus familiar with his presumable interests will have little doubt as to the great intrinsic value of this compilation. Others who from want of time or opportunity are debarred from personal attendance or examination will derive some secondary fire and enthusiasm for matters pathological by an inspection of the beautiful illustrations collected in the Atlas, and from reading the lucid explanatory letterpress interpolated between the plates. The latter are 136 in number and mostly coloured, and although chiefly illustrative of skin conditions they include many of general pathological interest. Medical men who have been unable by reading and clinical experience to keep themselves abreast of modern pathology will find these illustrations of skin conditions especially valuable. The older nomenclature has given place to a new order of things, and a conservative adherence to old names for old diseases is often accounted ignorance by those who have derived their learning in more modern schools.

Metals. By Huntington and McMillan. New Edition. Longmans Green and Co. 7*s.* 6*d.*

This book is based upon the work originally compiled by the late Prof. C. L. Bloxam in 1872, and re-edited and enlarged by Prof. Huntington in 1882.

The present edition has been still further enlarged, and for the most part brought well up to date, under the joint authorship of Prof. Huntington and Mr. McMillan. In its present form, the book, although not full enough to satisfy the requirements of students who are taking up metallurgy as a speciality, will be found of great use to the chemical student and others who are desirous of gaining a rather

fuller knowledge of the details and technicalities of metallurgical processes than are to be found in chemical text-books. The methods of "Coking" have been brought well up to date by the introduction of a description of the newest form of Semet-Solvay oven; and the section devoted to gaseous fuel and regenerative furnaces is much to be commended. It is to be regretted however that with the exception of a somewhat scant description of the electrical method for the production of aluminium no mention is made of this most modern of metallurgical engines, the electric furnace.

Die Morphologie und Physiologie des pflanzlichen Zell Kerne Eine Kritische Litteraturstudie. Von Prof. Dr. A. Zimmermann. Mit 84 Figuren im Text. Jena: Verlag von Gustav Fischer, 1896.

The author of this work has laid the general Botanical, and indeed Zoological, public under a debt of gratitude by producing an epitome of the multitudinous papers which have appeared on the subject of the vegetable cell-nucleus. We say *Zoological* public advisedly, because the results of their co-workers in the field of vegetable cytology are too often ignored by those who make animals their special study. But if there is one fact which stands out more clearly than another in the history of modern cytological investigation, it is this one, namely, that it is impossible, as well as unprofitable, to generalise either from the purely animal or the purely plant side in these inquiries. True it is that the partnership between Botany and Zoology has long ago become practically divorced, but that was but the inevitable consequence of the divergent paths followed in the evolution of the respective subjects of the two sciences. But the Protoplasm and Nucleus are structural entities common to both, and the conclusions arrived at on the two sides demand mutual completion and, not less, mutual checking. It is difficult enough to generalise soundly as it is, without going out of our way to avoid utilising the means of assistance which are accessible.

Dr. Zimmermann's book is one which will prove of great use as a guide to the literature on the plant side. It is very thorough, and on the whole seems remarkably free from the mistakes which it is very difficult to exclude from such compilations.

An account is given of the various histological methods in common use, together with their direct application to the study of the nucleus. The structure of the latter is discussed, and its behaviour during division is fully described. A few pages are devoted to a consideration of the physiology and function of the nucleus.

In the special part of the book, the nucleus is treated of as it occurs in plants belonging to the various divisions of the vegetable kingdom, and many interesting facts are here brought incidentally under review. An admirable bibliography forms by no means the least valuable feature of what we are glad to be able to characterise as an extremely useful book.

The Cell in Development and Inheritance. By Edmund B. Wilson, Ph.D. The Macmillan Company. 1896.

Professor Wilson's work is a clear and vigorous history of a province of biology which has become far better known and more interesting to the general scientific reader since the publication of Hertwig's Work, *Die Zelle und die Gewebe.* And it is perhaps not to be regretted that this new treatise on cellular biology is written from a decidedly Boverian and Weismannistic standpoint. Such a method brings out the cardinal points of the subject in far sharper outline before the general reader, than would be the case with any amount of analytical treatment, though this latter might be better adapted to the requirements of the few.

The introduction and the chapters dealing with the general characters of the cell—cell division—and germ cells, are all excellent, although in the description of the Mitotic Evolution perhaps undue importance is given to the centrosome as a cell organ. The general drift of experience seems rather to show that this structure is more the expression of the operation than the controller, of the intracellular forces.

The chapter on the conjugation in unicellular organisms is particularly good, and is in fact by far the most lucid *résumé* of the observations made on this complex subject, which is in existence.

It is only when we come to the chapters dealing with the vexed question of chromatin reduction in the generative cells that the work appears to be in any way open to serious criticism. The subject is at the present time entirely *sub judice*, and the attempt to treat it from a pronounced Weismannistic standpoint necessarily leads to an inflation of the importance of some observations, and the suppression of that of others, which is decidedly objectionable in our present state of knowledge. This is particularly apparent in the way the Author brings forward vom Rath's work on Salamander as lending much support to reduction in Weismann's sense, because this work has recently been shown by Meves[1] to be quite unsound.

The Spermatogenesis in Salamander completely corresponds with that described by Moore in Elasmobranchs and with Brauer's interpretation of the same phenomena in *Ascaris*. It is thus apparent that whatever the value of the process described in Copepods by Häcker, may eventually turn out to be, the universality of the "reductionstheilung" does not at present hold. The argument as based on the results of vegetable cytology is marred by the fact that the author does not always appear to be clear as to the standpoint of the authors he quotes, and in one such instance the respective facts are actually transposed; in fact throughout this chapter the author's endeavour to maintain his standpoint, although persistent, strikes us as being rather prejudiced.

[1] Ueber die Entwicklung der Männlichen geschlechtszellen von Salamandra maculosa. *Arch. f. Mikr. Anatomie*, Bd. 48. 1896.

The immediately succeeding chapters contains a complete description and classification of cell organs into persistent and non-persistent types of the different forms of spheres, and much interesting matter relating to the other constituents of the cell. In chapter viii we find a full description of "cell division and development" and in the succeeding chapters there is a very clear and readable discussion of the several theories of development, through all of which the author's own special knowledge is utilised in full, as in the passages dealing with the nature and causes of differentiation, the Promorphological relations of Cleavage and many more.

The whole work thus forms a most useful *résumé* of a large subject, the literature of which is so voluminous that it continually threatens to obscure the contained substratum of observations, and an epitome of this sort is a most important acquisition, as it brings the questions which legitimately spring up, when all the observations in each department are taken together, clearly into view.

Der Lichtsinn augenloser Thiere. Eine biologische Studie. Dr. Willibald Nagel. Jena: Gustav Fischer, 1896.

The author here reprints an interesting and useful lecture on "Sight without Eyes". This lecture, which takes up 48 of the 120 pages of his work, is, as the title signifies, a general account of what is known as the dermatoptic function. The second part gives a detailed account of the author's experiments on Molluscs, which, though blind (naturally or artificially), exemplify this function by reacting in various definite ways to the light stimulus. The third part is made up of appendices which are expansions of important points dealt with too briefly in the lecture. The work closes with a useful bibliography of some eighty references dealing with the physiology of sensation, chiefly of light. These range from Johannes Müller's classical *Zur vergleichenden Physiologie des Gesichtssinnes* (1826) to the author's own recent contributions.

From this outline of the contents it will be seen that the work does not pretend to be much more than a review and discussion of facts already known, *viz.*, that certain lower forms of animals without eyes have acquired the power of reacting to sudden changes in the intensity of light. As such, however, it is timely and interesting. Biologists will be grateful to the author for pointing out the interesting fact that those animals which possess hard, protective shells into which they can retreat and defy their enemies mostly react to sudden diminutions of light, *e.g.*, to passing shadows, such shadows no doubt warning them of the approach of a foe. On the other hand, animals which generally live under sand, from which they occasionally emerge, react on sudden brightenings of the light. The author distinguishes between these two different nerve reactions by the use of the words *Lichtempfindung* and *Schattenempfindung*.

The title of the work is certainly happier than the title of the lecture on which it is founded. It is quite true that both words "sight" and "eyes" are only sharply definable when used of human sight and eyes, and that, when the lower forms of organs for appreciating variations in light intensity are brought into line, the terms are often of more than doubtful applicability. But we do not think that this uncertainty as to what the comparative anatomist and physiologist, as apart from the human anatomist and physiologist, means by sight and eyes justifies the author in applying either term to the sensitiveness of certain skins to light. This, indeed, the author recognises, for on page 18 he modifies the word *Sehen* into *Lichtsinn*, and rightly adopts this modification for the title of his book.

Most people would be inclined to affirm that as the word "sight" was already in use, its meaning was fixed before we had any knowledge of the simpler eyes of the invertebrates, and that, while there can be no objection to calling all organs for the appreciation of light sensations "eyes," we cannot say that all the eyes *see* unless, from their possession of a dioptric apparatus or otherwise, we can conclude that they are capable of appreciating external form-differences.

The author writes as an authority, and all that is at present known of the remarkable sensitiveness to light possessed by the skins of many lower invertebrates (the list of which, by the way, he has not exhausted) he discusses with knowledge and ability. His treatise makes it perfectly clear that we are just as much in the dark as to the real causes of these diffuse light sensations of certain skins as we are of the true causes of the light sensation of our own specialised organ of vision. It is interesting to note that many of the surfaces sensitive to light, *e.g.*, the siphons of Psammobia, the skin of a "decapitated" Amphioxus, are absolutely free from pigment, but nevertheless the author admits that pigment must have some definite connection with sight, inasmuch as it is always present in specialised visual organs, except in albinoes, which are abnormal ("Missbildungen"). As to what this connection is, the author merely repeats the old suggestion that it prevents diffusion of the light rays by sheathing the visual cells, in order that they may clearly distinguish separate points of light. On the other hand, it may be pointed out that it is also always present in eyes which do not "see" in the restricted sense above sketched, and which do not require to distinguish separate points of light.

Cellulose. An outline of the chemistry of the structural elements of plants, with reference to their natural history and industrial uses. By Cross and Bevan. Longmans.

An almost inevitable result of the increasing activity of scientific investigators of the present day, and the consequent accumulation of scientific literature in almost every department, is the need for the appearance of definite monographs, in which, by specially qualified hands, the vast amount of crude material scattered here and there may

be concentrated into a more compact form, and so presented to the general reader. Messrs. Cross and Bevan in the present volume have brought together the sum total of the work done in recent years on cellulose, and have furnished a monograph, valuable alike to the chemist, the vegetable physiologist, and the reader interested specially in the economic questions associated with this substance.

But though at the outset entitled to great credit for so important a contribution alike to science and to art, it is to be feared that the gratitude of their readers will be largely tinctured with other sentiments, for the subject-matter is handled in a manner which is far from attractive. The book, though teeming with information, is emphatically dull, and likely rather to be used for reference purposes than to take its place among technical text-books.

The authors open their work by a somewhat exhaustive discussion of the characters, both chemical and physical, of the typical cotton-fibre, which is the purest form of cellulose known. They examine closely the behaviour of this body with numerous re-agents with the view of elucidating, as far as they can, the composition of its molecule. The results obtained, however, both by themselves and by other investigators, do more at present to show how extremely complicated the whole question is than to give us very definite information.

Dealing with the problem of the synthesis of cellulose they put forward the view that it is built up of molecules of simpler carbohydrates, and they quote two processes which throw some light upon the subject. When the juice of the beet is extracted and allowed to stand for some time a change takes place in it apparently spontaneously, resulting in the separation of lumps or clots of a white insoluble substance which has all the characteristics of cellulose, and when this is separated by filtration, the solution gives with alcohol a gelatinous precipitate which resembles the hydrates of that body. If the lumps first noticed are transferred to a solution of pure cane sugar a further formation of cellulose can be observed. The process is sometimes attended by the evolution of carbonic anhydride and by the formation of a certain amount of acetic acid. It is suggested that the process is due to the action of an enzyme allied to diastase.

The second process of synthesis is due to the action of an acetic ferment, which has been described by A. J. Brown in the *Journal of the Chemical Society*. The organism is composed of bacterial rods which are contained in a membranous envelope. Cultivated in solutions of levulose, mannitol and dextrose, it produces a vigorous growth composed of the bacteria enveloped in a "collecting medium" of cellulose. Levulose seems to be the most useful antecedent.

The interest of the vegetable physiologist will be perhaps most excited by the chapters on the so-called "Compound Celluloses". Of these the authors discuss the ligno-celluloses, pecto-celluloses, muco-celluloses, and adipo- and carbo-celluloses. The modifications of the cell wall which are especially characteristic of wood, mucilage, and cork, have of course

very great importance from their relationship to the physiological division of labour of which the plant tissues are the seat.

The authors go very exhaustively into the relation of these bodies to pure cellulose and establish the view that the latter is the antecedent of them all, being converted chemically into them. The hypothesis therefore of the formation of lignin, suberin, etc., in the cell and its wall becoming impregnated with them by some kind of infiltration is no longer tenable. The question, however, of the details of the transformations, or even whether they came from pure cellulose or some compound built up with it in the ordinary course of elaboration, the authors prefer for the present to leave undecided.

Perhaps the most interesting part of the volume, both to the student and the general reader, is the section dealing with the applications of cellulose in the arts. To the former class of readers, pages 243-273 will appeal very strongly, as the authors give very valuable laboratory and research notes dealing with the separation of cellulose from the various vegetable tissues, the methods of analysis, the characters of cellulose compounds, etc. The latter portion of the volume is more adapted for general reading, as it embraces a summary of the economic uses of the material. Many of the criticisms of the writers on these points are of the greatest value, particularly the section devoted to the consideration of the manufacture of paper from different celluloses. A fuller examination of these points is beyond the scope of the present notice, but the section in question brings out very grave considerations as to the permanence of many of our records.

The book is illustrated by fourteen plates which are reproductions of microphotographs of sections showing the sources of most of the celluloses of commerce.

APPENDIX.

NOTICES OF BOOKS.

Catalogue of the African Plants collected by Dr. Friedrich Welwitsch in 1853-61. Dicotyledons, part i. By William Philip Hiern, M.A., F.L.S. London: Printed by order of the Trustees of the British Museum, 1896.

The late Dr. Welwitsch was not only an accomplished botanist, he was a most discriminating and painstaking collector, and his collection of some 5000 species of Angola plants was a monument of skill and industry. It was a model of neatness and order, and most of the species were represented by a numerous series of specimens, accompanied by copious descriptive notes and observations made on the spot. The preparation of this herbarium occupied him upwards of seven years, and was accomplished under great difficulties and privations, to say nothing of severe attacks of fever and other diseases which few travellers in tropical Africa escape. He travelled in the service of the Portuguese Government, and in 1863 was permitted to come to England to work out his collections. The remainder of his life was devoted to this purpose; yet he published comparatively little; partly in consequence of the extremely critical nature of his work, and partly in consequence of almost continuous bad health. He died in 1873, and after some litigation with the Portuguese Government, his executors secured a very full set of his plants and a transcript of all his notes for the British Museum. Soon after Dr. Welwitsch settled in London Professor D. Oliver's *Flora of Tropical Africa* was commenced, and Welwitsch generously allowed the use and incorporation of his plants—almost the only ones known from Lower Guinea. This was continued, though imperfectly, through volumes i. and ii., and then interrupted.

Mr. Hiern's *Catalogue* is the first part of a work intended to embody Welwitsch's notes and descriptions, and his own descriptions of any unpublished species he may find in the course of his investigations. The part issued includes the orders Ranunculaceæ to the Rhizophoraceæ. It is preceded by a portrait and biographical sketch of Welwitsch, and a bibliography of his published writings and of other writers' publications, exclusively or chiefly devoted to definite parts of Welwitsch's collections. That Mr. Hiern has done his work very thoroughly is no more than would be expected from his previous performances; but there appears to be no mention of the language in which the notes and descriptions were originally written. This was Latin, and it is a question whether it would not have been better to have published them in the original, seeing that the collections were distributed among the herbaria of various countries. However, it is a great thing to have this treasury of informa-

tion accessible to the world. "Catalogue" must be regarded as an official rather than appropriate title, inasmuch as the compiler himself contributes descriptions of seventy new species and two new genera. The latter are *Epinetrum*, an imperfectly characterised genus of Menispermaceæ, of which only the male flowers are known; and *Zanha*, Burseraceæ. With regard to nomenclature, Mr. Hiern is a staunch follower of Dr. O. Kuntze, except where we can find an older name, no matter how obscure or barbarous; but there is no doubt a sincere attempt at consistency, and, as current names are cited, it is not so difficult to follow as some recent American works, in which obscure names replace familiar ones in the absence of synonymy.

A Manual and Dictionary of the Flowering Plants and Ferns. By J. C. Willis, M.A. Vol. i. (pp. xiv., 224) = part i. Vol. ii. (pp. xiv., 430) = part ii. Cambridge Natural Science Manuals. University Press, 1897. 10*s.* 6*d.*

This book may be said to consist of part ii. and part i. The author found, as he tells us, that part ii. (the Dictionary) was "wanting in co-ordination," and so wrote part i. (the Manual) "to supplement it". One hardly expects to find "co-ordination" in a dictionary, nor is it easy to see how this quality can be introduced by writing "a kind of index" to the said dictionary, even when this co-ordinating supplement or "kind of index" evolves during the writing into "what is practically a text-book of morphology, classification, natural history and geographical distribution". But let us leave the logical peculiarities of the book's inception and endeavour to judge each part on its own merits. Part ii., then, is intended to supply in the dictionary form "as much information as is required by any but specialists, upon all those points — morphology, classification, natural history, economic botany, etc.—which do not require the use of a microscope". Now the want of a book of this kind has been felt by botanists for some years, and part ii. of the present work certainly goes some way towards supplying it. The only question is whether it would not be a great deal more valuable as a book of reference for botanists if it were very considerably larger. But it appears that the author has particularly in view the needs of the elementary student "when placed before the bewildering variety of forms in such a collection as that at Kew," and these necessitate the restriction of the work "within a reasonable compass". Of course it is very easy to criticise such a work from an adverse standpoint—omissions there are which we should like to see repaired, but on the whole the book (*i.e.*, the second volume) will, without doubt, abundantly justify its existence, and prove a great boon to all those for whose help it has been written.

Part i. has to be judged on a very different footing. It is supposed to be intelligible to "any one who has studied any of the small elementary treatises on Botany," and chapter i. (more than half the volume) is avowedly intended to be read by a student in his first year of botanical

study. The amount of information crammed into the hundred odd pages of closely printed matter (relieved by but one solitary diagram) contained in the first chapter is positively appalling; nor can the lucidity of the author's style be said to compensate for the congestion of his matter. We cannot refrain from quoting an example in justification of this remark. Aerial roots, the author says, "are usually formed upon stems or leaves; such roots, together with branch roots formed upon other roots but not in acropetal succession (see below), are classed under the term of *adventitious roots*, whilst the original root derived from the embryo together with its normal branches (those formed in acropetal succession) are spoken of as *true roots*" (p. 21). We heartily condole with the student who attempts to assimilate this clumsily prepared morphological pemmican. And to make the matter worse, the author by no means confines himself to boiling down matters of fact and of accepted morphological doctrine, but under the plea of giving life to the dry bones of morphology by an "evolutionary" treatment, he introduces a large amount of highly speculative matter which does not escape the necessary dogmatism of the whole.

Of inaccuracies there are, as was to be expected, but few; and those almost entirely where the author ventures upon ground which he has wisely forbidden himself in the Preface—namely where he discusses topics which "require the use of a microscope".

The part of chapter i. dealing with the "Natural History of the Flower," and chapter iii., which deals mainly with "biological" groups of plants, contain useful if somewhat disjointed summaries of existing knowledge on these subjects; and the same remark may apply to that part of chapter ii. which deals with the "Principles of Classification". But we must protest against the section on "Variation" in this chapter. There was no need for the author to attempt to give an account of the methods employed in the statistical study of this subject, but having done so he might have taken the trouble to avoid the misstatements and confusions which literally crowd the single page devoted to this topic.

It is to be hoped that in future editions we may see part ii. somewhat expanded, and issued by itself, for we cannot approve the scheme of part i. (entirely apart from the question of its execution). If the author wishes to write a treatise on certain branches of botany, let him devote the necessary time and thought to its production. An adequate treatment of the subjects indicated in the preface would fill at least three times the space which the writer has given them. But do not let him imagine that he is benefiting anybody, least of all the elementary student, by writing what we must reluctantly call a cram-book on some of the most fascinating and important topics in the range of modern botany.

Estudios sobre la Flora de las Islas de Juan Fernandez. Por Federico Johow. A espensas del Gobierno de Chile. Santiago de Chile. Imprenta de Cervantes, 1896. 4to, pp. xi. and 287, with two maps, eight engravings in the text, and eighteen plates.

This is a valuable contribution to the literature on Insular Floras;

its value depending almost entirely upon the fact that it embodies the results of actual observation, instead of being drawn up from herbarium specimens, supplimented by the more or less imperfect notes of collectors.

With regard to numbers of genera and species, it contains very little that is not in Hemsley's account of the Flora in the Botany of the *Challenger* Expedition. But it embodies all that that contains, with very much additional biological, local geographical, and historical matter. It has the double advantage of being a consolidation of the work of a botanist who had access to Bertero's types and all previously existing literature, with that of a botanist whose investigations were conducted in the field. The author draws largely on Hemsley for synonymy and general geography, and is careful to cite his authority in every instance. The enumeration is preceded by an account of the geography and geology of the islands; a somewhat detailed history of their botanical exploration; and a full and interesting bibliography; and it is followed by an analysis of the flora; a description of the vegetable formations; catalogues of the insects and birds; and an appendix on the products of the animals, and the management and conservation of the woods and plantations. Dr. Johow has reduced a number of doubtful species, and in return he describes about half a dozen as new. Notable amongst the latter are *Centaurodendron dracænoides* and *Dendroseris gigantea*. The former is remarkable in the tribe *Cynareæ* for its aboreous character, and the latter for its size—up to twenty feet high, among the cichoriaceous arboreous Compositæ. The total number of endemic species of vascular plants resulting from Dr. Johow's elaboration is sixty-nine as against seventy given by Hemsley; and these belong to fifty-two genera, whereof twelve are endemic, and to thirty-two natural orders. In addition, there are seventy-four indigenous but not endemic species. Reference has been made in SCIENCE PROGRESS[1] to the sandal-wood tree of Juan Fernandez. Dr. Johow succeeded in finding one tree, after weeks of fruitless searching.

The beautifully executed plates, illustrating the scenery and characteristic types of vegetation add greatly to the interest and value of the book.

The Theory of Sound. By Lord Rayleigh. Second edition, revised and enlarged. 2 vols., 480 + xiv., and 504 + xvi. pp. London: Macmillan & Co., 1894 and 1896.

There are three works in the English language on Natural Philosophy which occupy quite a unique position; these are Maxwell's *Electricity and Magnetism*, Kelvin and Tait's *Natural Philosophy*, and the work under notice. For in every branch of physics which has so far progressed as to be treated by the methods of the mathematician we continually make use of the results and investigations contained in

[1] I., p. 401.

these three books. There is another striking resemblance between them in that in each case while free use is made of the help of mathematics the *physical* bearing of the problems and the *physical* interpretation of the results is everywhere given a prominent place.

In a notice of the first edition of Lord Rayleigh's *Sound* von Helmholtz concludes as follows: "At the end of the volume Lord Rayleigh has placed the words 'The End'. We only hope that this may be only the provisional, not the definite end. There is still an important chapter wanted, *viz.*, that on the theory of reed pipes, including the human voice. . . . After reed pipes we would mention the theory of singing flames and the blowing of organ pipes." In this second edition we have a fulfilment of Helmholtz's wish, for in the three hundred and fifty odd pages of additional matter the author has included discussions on these points.

The first edition is so well known to all physicists that it will only be necessary to refer to the additions which have been made in this edition.

Throughout the volumes short paragraphs and steps in the mathematical reasoning have been here and there added, and what is of much importance references to recent works on the subject have been included. The first considerable addition is a discussion on the vibrations of bells, in which the author describes the results of the experiments he has conducted in this subject. He has examined a number of church bells, determining in each case the different tones the bells are capable of yielding, and has been led to the interesting result that in English bells it is the fifth tone in order which agrees with the nominal pitch of the bell, while in Belgian bells it is the second tone that agrees with the nominal pitch. The author then goes on to investigate the vibration of curved plates and shells, the theory being only strictly applicable when the thickness is infinitely small.

From the author's well-known investigations on the telephone one naturally looks for a chapter dealing with this instrument; and in this connection there is also one on alternating currents, including the passage of alternating currents through a network of conductors, and the resistance of conductors to alternating currents. In connection with the measurements of the current needed to give an audible sound in a telephone given in the text, and also with reference to the minimum amplitude of sound waves that are audible, it is interesting to recall some recent measures by Barus who showed that the amplitude of the excursions of a telephone diaphragm is less than 10^{-6} cm.

Two very interesting sections are added dealing with the repulsion of resonators and the attraction between a vibrating body, such as a tuning fork, and a light suspended body. Incidentally the author considers the tendency of a suspended disc to set itself at right angles to the direction of alternating currents of air. Boys has shown to what an extraordinary degree the sensitiveness of such a disc as a detector of response in a resonator can be carried, and it is rather astonishing that

this arrangement has not been used in connection with a series of resonators to estimate the *intensity* of the overtones of notes produced by various instruments. The author in this section also gives an account of A. König's explanation of the transverse ridges in which cork filings in a Kundt's tube set themselves. The explanation depends on the fact that two spheres at a moderate distance apart in an alternating current will, on account of the difference in the velocity of the air between them and that along their averted hemispheres, repel one another if the line joining the centres is parallel to the direction of motion of the air, and will attract one another if the line joining the centres is perpendicular to the stream of air. The result of these forces being a tendency to aggregate in transverse laminæ.

In the chapter dealing with the vibration of organ pipes the author, while mentioning the observations made by Töpler and Boltzmann, in which the optical interference between two rays of light, one passing at the side of the pipe and the other through the air in the pipe, which, on account of the vibrations, is alternately compressed and rarified, does not mention the beautiful results obtained by this method by Raps (*Wied. Ann.*, 50, 193, 1893).

The subject of singing flames and such like, in which the vibrations are maintained by heat, is a most fascinating one. The complete explanation of the phenomena on account of the peculiar and ill-understood behaviour of the flames being a matter of considerable difficulty. The author discusses Sondhauss's experiments in this subject, and shows how the effects may be explained on the supposition that stationary vibrations are set up in the gas supply tube, the phase of which may be either the same or opposite to that of the vibrations in the resonator. Since it is probable that the greatest development of heat is slightly retarded in comparison with the issue of gas, there will be a tendency, according to the difference in phase, either to maintain or to damp out any existing vibrations in the resonator. The vibrations set up in glass bulbs when heated, a phenomenon familiar to all glass-blowers, is another example of vibrations maintained by heat, and in this connection the author might have considered the very interesting thermal engine exhibited, we believe for the first time, by Mr. Griffiths at the meeting of the Physical Society held in the Cavendish Laboratory a year or two ago. It consists of a glass bulb about 6 cm. in diameter connected to a U-shaped tube ot about 1 cm. bore. The U is filled with mercury for about 6 cm. up each limb and the bulb heated over a flame. If the mercury is disturbed by plunging a rod into the open limb of the U, oscillations are set up which increase in amplitude to a most astonishing extent, and are maintained as long as the heating continues.

The additional chapters, to which we have not space to refer in detail, deal with the effects of friction and heat conduction on acoustical problems, capillarity and capillary waves and ripples, vortex motion and sensitive jets, vibrations of solid bodies, and finally a long chapter

entitled facts and theories of audition. In his preface the author states that this last chapter has entailed labour out of proportion to the results, but here we do not agree with him, and fancy that in this all students of Physics will agree with us.

A notice of such a work as Lord Rayleigh's must of necessity partake of the character of a table of contents for the only thing that can really be said about it is to regret that the work is not larger, and to express a hope that the author will himself attack some of the problems which he indicates as awaiting a satisfactory solution.

In conclusion we need only say that this edition is practically the publication of a third volume and must be invaluable to all physicists.

Magnetic Fields of Force. By Professor H. Ebert. Translated from the German by Dr. C. V. Burton. Pt. i., pp. xviii. + 297. London: Longmans, Green & Co., 1897. 10*s.* 6*d.* net.

In this volume the phenomena of magnetism and electricity are developed according to Faraday's conception of lines of force. In Maxwell's famous book on this subject the advanced student possesses a complete treatise; to those, however, who have not got a great command of mathematics this work is of very little use. In Professor Ebert's book we have the subject dealt with in a clear and concise manner and without the assistance of any mathematical reasoning which is above the understanding of a student acquainted with quite the elements of this subject. In part i. the more elementary portions of the subject are dealt with, the consideration of the phenomena of induction, single and multiphase periodic currents and electrical oscillations being reserved for a subsequent volume.

Throughout the volume directions are given for performing experiments suitable for lecture demonstration, or for the use of elementary students in the laboratory. These directions are rather detailed and in most cases are easy of performance. On page 97, however, in *Experiment* No. 40, the reader is told to suspend a magnetic needle by fastening to its middle a fine fibre, and is told that it will set itself in the direction of the earth's magnetic lines of force, no warning being given as to the difficulty which would be encountered in avoiding the disturbing effect of gravity if the needle is not supported exactly at its centre of gravity. The experiment then proceeds: "Now disturb the needle from its position of rest and allow it to oscillate. From the frequency of oscillation we can calculate **T**, the resultant intensity of the earth's magnetic field, or *total force*." This is quite wrong if we take it literally, as from what is said in the next section about deriving the value of **H** from that of **T** obtained as above, or if the author means that from the period of oscillation *and the value of the dip* **T** can be calculated we can only say he has managed very successfully to obscure his meaning.

In a work such as this where all the magnetic phenomena are considered as due to something going on in the ether it is rather

astonishing to find that all the dimensions are given subject to the assumption that the permeability of the medium is without dimensions being merely a ratio. The inclusion of the permeability and specific inductive capacity in the dimensional equations seems as if it would never find its way into any elementary text-books.

The translation is everything that could be desired, the English being clear and not defaced by the German construction of the sentences so often found in translations. Dr. Burton has added here and there explanatory sentences (indicated by square brackets). In one or two places, however, these additions might have been considered as *corrections* and introduced into the text, for at present there are a few rather laughable sentences formed by the combination of the translator's addition with the author's original statement. Thus on p. 277 we read: ". . . A current of one ampère produces at a distance of 1 cm. from its axis a field-intensity of $\frac{1}{2}$ of an absolute unit, supposing the permeability to have the value unity [or any other value]".

Physiologische Pflanzenanatomie, von Dr. G. Haberlandt, O. Ö. Professor der Botanik, vorstand des Botanischen Institutes und Gartens a. d. k. k. Universität Graz. Zweite, neubearbeitete v. vermehrte auflage, mit 235 Abbildungen. Leipzig, Wilhelm Engelmann, 1896.

The publication of Dr. Haberlandt's *Physiologische Pflanzenanatomie* some twelve years ago marked a new departure in the general treatment of anatomical facts. Naturally the author did not himself actually initiate the movement of which his book was the outcome, but he was perhaps the first who gave a clear and general exposition of the old facts regarded from the new standpoint, and the results have fully justified his efforts. Anatomical characters, no doubt, oftentimes possess an intrinsic importance of their own, even from a taxonomic point of view; that is, they may possess even a high value in certain cases, as in a measure indicating phylogenetic affinities. But no one will deny that in the vast majority of cases one is on very unsafe ground in attempting to show phylogenetic conclusions from such a source. And this being so, it is obvious that plant anatomy is in some degree robbed of an interest which renders the study of the comparative anatomy of animals so interesting. But, on the other hand, the botanist gains on account of the great extent of individual (*i.e.*, within the same species) variation exhibited by plants, and the marked correlation which may be seen to exist between the organism and the special kind of environment in which it happens to be located. A plant cannot often grow where it will but where it must. Hence the continuous need of a possibility of variable response to conditions which themselves vary. Thus it is that plant anatomy is so closely bound up with the circumstances of the passive environment, and that its study, from this point of view, constitutes one of the most fascinating branches of investigation, although the ulterior question as to how it is possible for an organism to have become possessed of the power of responding in a purposeful

way to this *or* that set of conditions is still a mystery, and is long likely to remain so in spite of the assurance to the contrary of sundry writers on these matters.

The new edition of Professor Haberlandt's work is a greatly enlarged and in most respects a greatly improved one. The fact that the author has himself contributed in no small degree to our knowledge of the subject imparts to his book a freshness, and an interest to his method of treatment which only a first-hand acquaintance with the facts can give. Moreover, Dr. Haberlandt knows when he has said enough on any topic, and does not weary his reader with tedious detail, but rather stimulates him to prosecute further researches for himself.

Without doubt the book in its present form will, as it thoroughly deserves, enjoy a wide reputation, safe of welcome recognition at the hands of all who are competent to form an opinion as to its merits.

The Ancient Volcanoes of Great Britain. By Sir Archibald Geikie, D.C.L., F.R.S., etc., Director-general of the Geological Survey of Great Britain and Ireland. 2 vols., pp. 477 and 492 with 7 maps and 383 illustrations. Macmillan & Co., 1897.

It is impossible to rise from the perusal of this work without being impressed alike by the prodigious industry of its author and by the remarkable literary skill with which he has utilised the vast mass of materials that he has collected. The work, which as now presented to us is almost encyclopædic in character, is an expansion and development of two valuable addresses delivered by Sir Archibald Geikie in 1891 and 1892 as president of the Geological Society, and of a memoir on the Tertiary Volcanoes of Britain contributed by him to the Royal Society of Edinburgh in 1888.

During his long connection with the Geological Survey, and in the course of his labours in mapping portions of our islands, especially in Scotland, the Director-general has enjoyed special opportunities, as he informs us, for studying the numerous rocks of volcanic origin associated with the whole series of British strata. In the present work he has brought together his own observations with those of his fellow-members of the Survey, and of other geologists, bearing on the history of Volcanic action in the British Islands, and he has combined the whole with such striking literary ability as to make the work as readable and intelligible as it is accurate and painstaking. Nor is the book, though avowedly based on publications already familiar to geologists, wanting in original matter; many new observations—especially those made during a yachting cruise in the Western Isles of Scotland—with numerous fresh illustrations and not a few valuable contributions from the field-geologists, palæontologists and petrographers of the Geological Survey repay the readers of this book who are already familiar with the author's earlier publications. An especially valuable feature of the book consists in the number and accuracy of the references to the original sources of information which it contains.

The first division of the work, running to somewhat over one

hundred pages, is devoted to a general sketch of modern volcanic action and a comparison of ancient volcanoes—as illustrated by their denuded relics embedded among the sedimentary deposits of past ages—with those of the present day. The author then proceeds to sketch successively the evidences of volcanic action in the British Islands, in Pre-Cambrian, Cambrian, Silurian, Devonian and Old-Red-Sandstone, Carboniferous, Permian and Tertiary times. Convenient as is this strictly chronological order from many points of view it is attended with the disadvantage that the writer is compelled to proceed "from the unknown to the known"—from the study of the much altered and greatly denuded fragments of old volcanic rocks, in which the relations with modern deposits are often very obscure to the comparatively fresh and well-preserved relics of Tertiary Volcanoes, in which the analogies with existing volcanic masses are so clear that they cannot be misunderstood." Apart from this obvious objection to his method, however, all geologists must feel indebted to the author for the way in which he has succeeded, by weaving together facts and observations from sources new and old, in giving a connected and up-to-date account of all that is known concerning the volcanic history of Britain during each of these ancient periods.

The last chapter of the work, consisting of about a dozen pages, is devoted to a "summary, and general deductions". It is here, if anywhere, that the reader may perhaps experience some little disappointment. In the various memoirs on volcanic phenomena—the publication of which has extended, as the author reminds us in his preface, over a period of nearly forty years—it naturally happens that we are able to trace many changes of opinion or rather developments of the author's knowledge; there is indeed very little in common between the conclusions arrived at by him during the first half of that period and those expressed during the latter half. As Sir Archibald Geikie has frequently to admit in the course of the work, the views he now expresses are, on many points, strikingly divergent from those which he formerly maintained, while certain of those views are in not less marked conflict with the conclusions of some of his contemporaries. Many geologists will, we think, demur to the new analysis and classification of volcanic phenomena presented in the pages of this his latest work. But concerning the great value of the book—embodying as it does a vast amount of original observation, patiently collected by himself and numerous fellow-workers in the same field, and skilfully woven into a connected and amply illustrated narrative of the succession of events in past geological times—there cannot be two opinions. Apart from all theoretical considerations, the author must be congratulated upon having produced a treatise which will be of great and permanent value as a book of reference, and, at the same time, of a literary masterpiece which cannot fail to add to his already high reputation as an exponent of science.

APPENDIX.

NOTICES OF BOOKS.

The Elements of Physics. By Edward L. Nichols and William S. Franklin. Vol. iii. Light and Sound. The Macmillan Co., 1897.

This is the third and concluding volume of a text-book, the second volume of which was noticed in SCIENCE PROGRESS (April, 1897). In this volume the two branches, Light and Sound, are considered together under the general head of wave motions which affect the sensory nerves. The remarks made with reference to the second volume apply in the case of the third volume, with the exception that in this volume the references have been put in.

Studies in Chemical Dynamics. By J. H. Van't Hoff. Revised and enlarged by Dr. Ernst Cohen. Translated by Dr. Thomas Ewan. Amsterdam: F. Muller & Co. London: Williams & Norgate, 1896.

The first edition of Professor Van't Hoff's *Etudes de Dynamique Chimique* being exhausted, the question of issuing a new edition was suggested, but the difficulty arose that the *Etudes* represented the stage of the author's chemical thinking at the time of their publication about twelve years ago, but that now-a-days most thinkers have passed beyond this stage. To make many changes in the new edition would, however, have taken away from the work its desired documentary character, while to change nothing would have contributed nothing towards making the contents acceptable to the public. Under these circumstances Dr. Cohen undertook to revise the *Etudes*, so as to include not only the old matter, but also an account of the researches which have been engendered by it, so as to form a record of the work carried out in Van't Hoff's laboratory.

The book is divided into four sections dealing with (1) the course of chemical change, (2) the influence of temperature on chemical change, (3) chemical equilibrium, and (4) affinity.

In each case, in addition to a complete account of the experiments which have been made, and the apparatus used, there is a theoretical discussion, the experimental facts being used to test the accuracy of the theory. It must not be supposed that the experiments made in Van't Hoff's laboratory are exclusively referred to, references are given to work bearing on the subject done elsewhere, and the results obtained are quoted.

The translation is all that could be desired, and the book will be of great use to English-speaking chemists and physicists both as showing in a clear and striking manner what has been done in the subject, and as a source from which many instructive and probably fruitful suggestions may be obtained.

The Induction Coil in Practical Work. By Lewis Wright. pp. 172. London: Macmillan & Co., 1897.

The enormous number of induction coils which during the last years have come into general use, the users in many cases having no previous experience in electrical matters, makes the publication of a book giving practical direction as to the use of a coil of some value. Mr. Wright not only describes in popular language the modern induction coil, and the practical details necessary to apply it to the production of Röntgen rays, but also gives descriptions of other experiments which can be performed with such a coil. Thus there are chapters on discharges in partial vacua, spectrum work, and discharges in high vacua. No doubt the book will be found of great service, particularly to medical practitioners who are attempting to apply Röntgen rays to diagnosis.

Elements of Electro-Chemistry. By Dr. Lüpke. Translated by M. M. Pattison Muir. London: H. Grevel & Co., 1897.

This work is intended to give those who are not in a position to make an exhaustive study of the subject an insight into the modern theories of electro-chemistry and of the experiments by means of which these theories are supported. The various laws are throughout deduced directly from the result of experiment, and mathematical discussions are only very sparely introduced. The experiments, which are very fully described, are of the nature more of lecture experiments than of exact quantitative determinations. Hence it follows that the book is more suited as a handbook for use in a laboratory where the students perform elementary physico-chemical experiments, or for the use of a teacher who wishes to illustrate his lectures by experiments, than as a text-book for the use of students anxious to know how the results have been actually obtained.

The first part of the book deals with the modern theories of electrolysis, and in this section we find a consideration of Hittorf's experiments on the velocity of the ions, Kohlrausch's work on the electrical conductivity of dilute solutions, Arrhenius's theory of dissociation. The second part deals with Van't Hoff's theory of solutions, and contains chapters on osmotic pressure, vapour pressure of solutions, boiling and freezing points of solutions, the connections between osmotic pressure, vapour pressure, increase of boiling point and lowering of the freezing point, and on the applications of Van't Hoff's law to solutions of electrolytes. The third part deals with the osmotic theory of the current in galvanic cells. This section includes a chapter on Nernst's calculation of the E. M. F. of cells from the osmotic pressure theory. In the next three chapters, which are on concentration cells, Daniell cells, and reduction and oxidation cells, the formulæ, etc., obtained in the first chapter of this section are applied. There are also chapters on the solution pressure of the metals, intensity of fixation and polarisation, irreversible cells, accumulators, and the energetics of galvanic elements. The last chapter deals with so

important a part of the subject that its meagre and scanty treatment of the subject of energetics is much to be regretted.

Now that chemists are everywhere measuring physical quantities, it seems a pity that they cannot make up their mind to use the C. G. S. system of units in its entirety, so that one would not find work measured in "litre-atmospheres" as appears at one part of the book. In places the terms ampere and volt are muddled up. Thus on p. 156 we read of a *current* of 72 *volts*, while in giving directions for performing some of the electrolytic experiments the current is defined in terms of "accumulators" without any mention of the resistance of the circuit. As an experiment to show that the direct contact of metals has very little effect in developing electro-motive force, the author says: "Copper wires are soldered to a nickel and a silver wire, and these wires are laid on a galvanometer; when the wires are thoroughly cleaned and then pressed one on the other, the needle remains at rest". Of course the needle remains at rest; in this form of experiment it would do so, however great the contact E. M. F.

The name of the translator is sufficient guarantee that this part of the work has been efficiently done.

Annotationes Zoologicæ Japonenses, Auspiciis Societatis Zoologicæ Tokyonensis. Seriatim Editæ. Vol. i., parts i. and ii. Tokyo, 25th May, 1897.

The above-named publication marks a new departure in the advancement of Zoology in the Far East. As explained by Professor Mitsukuri in an admirable historical introduction, written in choice English, it is an extension of the "foreign language part" of the *Zoological Magazine of Japan*, now in its ninth volume—in effect it is an *Anzeiger*, in which articles will be admitted in English, German, French and Italian, primarily by way of relieving the *Journal of the College of Science of the Imperial University* of less formal or preliminary notices and of making the progress of Zoology in Japan known abroad. We heartily congratulate our Eastern friends upon so heroic a course, and recommend this to workers in certain other countries which stand in a somewhat similar relationship to the great European nations, who are at present disposed to retard progress by writing only in their mother tongue. In detailing the events which have led up to the foundation of the *Annotationes*, Professor Mitsukuri emphasises the interesting fact that as early as the eighth century in the Christian era there was already established in Japan a University with four departments; and in tracing the influence of knowledge upon the destinies of his native land, and upon its scientific productiveness which is of quite recent date, he attaches much importance to the efforts of a band of earnest Japanese physicians who in the middle of the eighteenth century mastered the Dutch language, with a view to ascertaining something of western medicine. Proceeding to later events of historical interest, he deals as gracefully as gratefully with the foundation of the modern school of Zoology in Japan, in 1877, by Professor Morse,

and the introduction into it of modern methods of technique by Professor C. O. Whitman. Passing to subsequent years, he naïvely veils reference to his own share in the work in delicate allusions to his *confrères;* and it is for us to add that he will rank as the one man who gave the movement the final impetus which ensured success, and that no name will be more memorably associated with its history than his own. The body of the *Annotationes* contains six papers, of which three are wholly systematic, illustrated by three plates which, though somewhat crude, are truthful and expressive (*i.e.*, they are Japanese!) and by woodcuts, and there follow some "Miscellaneous Notes" and a report of the monthly meetings of the Zoological Society of Tokyo. The appearance of the names Mitsukuri, Ijima, Ishikawa, Goto and Kishinouye, among contributors and responsible promoters, is a sufficient guarantee of the value of the work; and the record of new and remarkable species of Chætognatha, of experiments on the accommodation of Infusoria to certain solutions, of the changes with advancing age in the calcareous deposits of *Stichopus*, with a revision of Hexactinellids with discoctasters, is a sufficient testimony to its breadth and novelty. The growth of holothurian "spicules" is a subject which has already occupied the attention of Japanese zoologists, and the afore-mentioned paper on the process in *Stichopus*, which is by Professor Mitsukuri, has an especial interest, since it furnishes support with extension to Australian forms to Théel's conclusion that *Holothuria armata* (Sal.) is but a form of *S. japonicus*, and suggests that the *bêche-de-mer* question would appear destined to final solution at the hands of an eastern zoologist.

The publication is a legitimate outcome of noble work performed for its own sake, and we wish it the success and support it deserves.

It is moreover anticipatory, as we gather from Professor Mitsukuri's remarks, to the transfer of the Marine Observatory at Misaki, which is now famous in the annals of Zoology in the Far East, a couple of miles northwards, with considerable enlargement.

For several years past the activity of Japanese biologists has been a subject for comment and admiration by the world, and be it remembered that it has lately yielded us one of the most important among post-Darwinian discoveries in Botany. From recent events the zoologists would appear to be following closely in the wake of their American brethren on the opposite shore of the North Pacific, in a manner amounting to friendly rivalry. Recent work upon the chimæroid fish *Harriotta*, the deep-sea gastropod *Pleurotomaria*, and the remarkable cephalopod *Opisthoteuthis*, justifies this assertion; and in view of it no little interest attaches to an announcement from the United States, which followed close on the publication of the *Annotationes*, that there would appear in June of the present year in connection with the *American Journal of Morphology* a *Zoological Bulletin.*

Professor Mitsukuri remarks in his introduction that the *Annotationes* will for the present be issued quarterly, and the editors of the American *Bulletin* anticipate that material will be forthcoming "for at

least six numbers a year". The one, as we have intimated, is, and the other it is promised shall be, of the nature of an *Anzeiger*, to contain but preliminary notices and contributions of a brief and desultory nature—*i.e.*, a running record of passing investigation. We beg of the promoters to adhere to their original scheme for both the contents and rate of issue of these most useful publications; and in the name of the teachers in our higher colleges and of responsible heads of establishments devoted to zoological research, who now-a-days are overwhelmed in the sacrificial task of endeavouring to keep pace with current literature, we exhort them to exclude lengthy dissertations, and to curtail to the utmost discursive and polemical ebullitions, since, by the introduction of such things, the scope of certain allied periodicals has become so enlarged that their original objects have been exceeded and their too rapid appearance has become a burden. And we sincerely hope they will not allow their pages to be frittered away in scrappy unillustrated diagnosis of new genera and species, begotten of a weak-minded craving for priority, such as are at times admitted wholesale into those of contemporary periodicals.

Lehrbuch der Zoologie. Von R. Hertwig. Vierte umgearbeitete Auflage. Jena: G. Fischer, 1897.

The present edition of this well-known work exceeds its predecessor in length by but thirteen pages. The general arrangement of its contents remains unaltered, and we still meet with the insufficient treatment of the vertebrata and scanty recognition of certain invertebrata so characteristic of the earlier editions. The leading modifications, as explained by the author in his preface, are the reconstruction of the chapter on the Sporozoa and the utilisation of Boulenger's classification of the Reptilia. The former is largely associated with an incorporation of the writings of Wasielewski, whose figures of *Coccidiam* replace those of Hatschek, and from which Labbé's *Hæmamœba laverani varietas quartana* is adopted. The Gregarinidæ are dealt with as an order of the class Sporozoa; and, conversely, the Ophiuridæ, in previous editions regarded as an order of the class Asteroidea, are now elevated to class rank. The Trilobita, hitherto with the Xiphosura and Gigantostraca disposed of in an *Anhang* to that portion of the work devoted to the Entomostraca, are now dealt with as an order of this sub-class by recognition of the epoch-marking discoveries of Beecher in *Triarthrus*. In effecting the latter radical change in his work by the incorporation of irresistibly important observations in palæontology, the author has laid himself under an obligation to similarly modify other sections of it which deal with groups of animals the inter-relationships of which have been during recent years rendered specially clear by the discoveries of palæontologists. Seeking the fulfilment of this, we note that the incorporation of Beecher's work is accompanied by a fuller recognition of the Rhynchocephalia, but of the allied "Eotetrapoda" of Credner and the wonderful series of anomodont reptiles which Seeley has during the last few years described, many of them being

annectant forms the importance of a knowledge of which is to the elementary student at least as great as that of *Triarthrus*, we find no mention. Nor is the palæontological portion of the book alone thus weak and behind the times. Two and a half years have elapsed since the publication of the previous edition of the work, and during that period it has been discovered that some Marsupials develop an allantoic placenta; and in *Anaspides tasmaniæ* there has been described a living Crustacean combining characters diagnostic of the Amphipods, Schizopods, and Decapods. Half a dozen investigators have during the period radically revolutionised our knowledge of the tooth-genesis of the Marsupialia; but of these and numerous other topics which, like them, are landmarks of contemporary investigation that have necessitated a reconsideration of first principles of classification and of our accepted views of phylogeny and structural relationship, we find no mention in the book. The remarkable treatment of the Bryozoa, Brachiopoda, and Tunicata, upon which we have previously commented, is still persisted in; and we would again point out that *Rhabdopleura* and *Cephalodiscus* are important forms which should be made known to even the elementary student, while annectant types such as *Acinetoides* among Protozoa and the Nothosauria among reptiles might well have been alluded to, when, on the author's own showing, pages dealing with the Protozoa and Reptilia have been materially rearranged. When we consider that the accumulation of zoological literature during the last twenty to thirty years has been of a phenomenal order, unparalleled by that of any other branch of science, and the consequent colossal nature of the task of compiling a text-book of the subject, we feel profoundly grateful to the author of the present work for what he has done. His book, however, good and sound so far as it goes, is thin, and we would have more of it, while more and better illustrations are sorely needed to do even justice to the text. Beyond the new illustrations already alluded to and some trivial modifications in others, there is introduced in the present edition but a drawing of a bird's skull. The replacement of Leuckart's time-honoured figure of *Distomum hæmatobium* by a far better, delineating with some degree of clearness the remarkable relationships of the female to the canalis gynæcophorus of the male, is indicative of promise for the future, for in the reproduction *ad nauseam* of antiquated illustrations our current text-books of zoology are not to be beaten.

The Life-Histories of the British Marine Food-Fishes. By W. C. McIntosh, F.R.S., and A. T. Masterman, B.A., B.Sc. London: C. J. Clay & Sons, 1897.

This volume of 467 pages is very welcome, and comes as the realisation of a long-cherished desire of all interested in fisheries work in Great Britain. It is defined by its authors as "a popularised epitome of the results achieved by British and foreign scientific workers at the St. Andrews Marine Laboratory and elsewhere". All our commercially important fishes, with other species to the total number of nearly ninety,

come under consideration, and by way of illustration there are twenty beautifully executed plates, largely coloured, together with a number of maps, woodcuts and tables. The work opens with a short preface, in which its objects and scope are first defined and a contrast is drawn between our present knowledge of the subject in hand and that of 1883, the year of the Great Fisheries Exhibition and of the Royal Commission on Trawling; and there follows an Introduction, in which there are considered the attitude of the public mind towards fishery questions, the present stage of the fishing industry in the evolution of human affairs, together with some historical and preliminary remarks, in which among other things the authors assert (not without justification) that in the past "authorities entrusted with the patronage of posts in which marine zoology could be studied, as a rule and with singular impartiality filled them with those accustomed to other departments of the subject, while men imbued with enthusiasm for marine zoology were stationed far inland". A great deal of emphasis is in various parts of the book laid upon the Trawling Commission afore-mentioned, with allusion amounting almost to reverence for the late 13th Earl of Dalhousie, its chairman, to whose memory the present volume is dedicated. The main contents of the book are sharply divided into two parts; a first and lesser part of 113 pages, which deals with the more general structure of the eggs, and the broader aspects of the life-history, development, and rate of growth of the fishes, and a second and greater part of considerably over 300 pages, in which the life-histories of the species of the several families are more fully considered. It is in the illustration of more especially the latter that the twenty plates have been drawn; and in that of the former there are a series of excellent woodcuts, which delineate the young stages of some of our more familiar food-fishes and were originally drawn in illustration of a famous lecture delivered by the senior author before the Royal Institution in 1889. When, in 1875, Professor McIntosh published his treatise on the *Marine Invertebrates and Fishes of St. Andrews*, expectation ran high, for it became evident that a master mind was intent on the systematic study of the rich fauna of that locality. The Royal Institution lecture was followed by the publication in the *Transactions of the Royal Society of Edinburgh* of an extensive monograph, in conjunction with Mr. E. E. Prince, now Dominion Commissioner in Fisheries, Canada, "On the Development and Life-histories of the Teleostean Food and other Fishes," perhaps the most generally interesting and important contribution to the subject that has yet appeared by British authors. Treatise, lecture, monograph, are now seen to have been stepping-stones in the formation of the book under review, while the numerous magazine articles and official reports which the senior author has in the meantime put forward have long led us to speculate upon what would be possibly forthcoming in it. The determination of the identity of the eggs and young of our familiar food fishes, found as they are among heterogeneous assemblages on the surface and in the shallower waters of the ocean, is a perplexing task, demanding great patience and technical skill, which during only the

last fifteen years has been systematically taken in hand by British workers, and in bringing together the sum of our knowledge concerning these this book marks an epoch in the advancement and popularisation of practical fishery affairs. Its only compeer of English origin is Cunningham's *Marketable Marine Food-Fishes*. It, however, transcends that, and on perusing it we confess to a feeling of pride in the amount that is really known and experimentally established, and of profound gratitude to all connected with St. Andrews for their noble share in the work.

The book has further a special interest since it is the first important publication which has emanated from the Gatty Laboratory of the University of St. Andrews, over which Professor McIntosh presides. The presentation of this building as a mark of appreciation of its director's epoch-marking labours in Marine Zoology is a matter of such recent history that it calls for no comment here, except to say that it marks an event upon which he and his ancient University are to be heartily congratulated. Professor McIntosh will be remembered in history as a pioneer in the application of Zoology to fishery affairs. The work of St. Andrews, with which his name and the volume under review will be ever associated, stands alone; it has been done on slender means, against considerable odds, and even now that in the establishment of the Gatty Laboratory it has received such deserving recognition from a private source, the miserable pittance allotted it under the Fishery Board for Scotland has ceased. Although the bulk of the observations set forth in the present volume have been made at St. Andrews, among leading investigators whose names occur in the text are Brook, Cunningham, Dunn, Fulton, Green, Haddon and Holt, than whom none have during recent years done more by honest labor to advance our knowledge of British Marine Fishes. The work of not a few foreign contemporaries of necessity comes in for recognition—Agassiz, Whitman, Dannevig, Möbius, Petersen, Sars—and the English Zoologist asked to recommend a series of reliable drawings extending to the later stages of development of the Plaice and Flounder is no longer under the humiliating necessity of referring his applicant to them and them alone. The weakest portion of the book is that dealing with organology and embryological detail. For want of better illustration a great deal of this will miss fire with the general public, to whom the usage of terms such as "periblast" and "intermediate cell mass," without sufficient definition, can convey little idea. Gross errors we have not detected, but among interesting topics of far-reaching importance we could have wished a record of the presence of one or more spiny rays in the anterior dorsal fin of the Hake; while further details concerning the appearance of similar structures, of cephalic spines, and the development of an air-bladder, among young Pleuronectids, and a mention of the existence, symmetry, and habits of *Psettodes* would not have been amiss. Fuller recognition might have been granted the investigations of Möbius concerning the renal origin of the thread of the Stickleback's nest, so interestingly associated with the contemporary researches of Prince; and a fuller account of the spawning act of the male herring

ought certainly to have been given. Where all has been so well done, however, there is little room for criticism. The book is well up to date, as passing reference to the researches of Grassi and Calandruccio on the life-history of the Eels sufficiently testifies; and its pages teem with detail, set forth in pleasant and enthusiastic terms wholly destitute of pedantry. Among those portions of the book deserving special mention as incurring the lasting gratitude of the student and practical worker are pp. 42-56 which deal with analyses of "pelagic life during the various months" of the year, and pp. 462-463 which bear a "synoptic table of the eggs of marine fishes"; and it may be remarked of the authors that they have utilised the practical fisherman to the best advantage, for example, as concerning the matter (p. 15) of the wholesale devouring of herrings' eggs by the haddock. Their whole attitude is that of devoted workers, men among men, anxious to press into the service of their cause all who are likely to aid it; and to this end due prominence is given to the statement of topics for immediate investigation. It is remarked on p. 66 that in the study of the subject in hand "the zoological investigator is stimulated by the fact that all his labours directly bear on the public welfare" and that although "the state has in past years spent princely sums on more or less pure science, . . . at the present moment the public interests demand . . . the exhaustive investigation of all that pertains to the food-fishes of our shores, since the problems connected therewith affect the prosperity of so large a portion of the population". While appeal is thus made to the state, an enthusiastic tone is struck which pervades the whole book. Under the spell of this the intelligent reader feels himself imbued with an ambition to work out some detail, if only in personal regard for authors who have so nobly bestowed time, labour, and money upon their chosen occupation. And when we consider how vast a field lies unexplored and how fascinating is the work to be done, pending the consideration of the state, we could desire nothing more for the authors than that their book should entice unto them the private workers whose aid they so fully deserve.

Das Ellenbogengelenk und seine Mechanik. Eine anatomische Studie, von J. W. Hultkrautz. Jena: Gustav Fischer, 1897.

This study of 144 pages, by the Prosector in Anatomy to the Karolina Medico-Chirurgical Institute of Stockholm, like so much in the domain of comparative vertebrate anatomy which during recent years has emanated from that city, bears the imprimatur of Professor W. Leche, since to him and Professor Lindström the author acknowledges his indebtedness for the material upon which it is based. It is divided into four sections, of which the first three deal minutely with the structure, movements, and development of all parts of the human elbow joint, accompanying tables being introduced where necessary, while the fourth is devoted to a comparative morphological study of the joint in representative members of the classes and orders of terrestrial vertebrata. The work is necessarily of a very technical character, and is begotten of a devotion to the cause of science nobly supported by Dr. Gustav Fischer

in its publication on his world-renowned lines. That portion of it which deals with the architecture and mechanics of the bones as involving an analysis of their "cancellous" constituents is, nevertheless, in a measure fascinating.

The work is illustrated by four plates and twenty-one woodcuts, and so far as we have been able to test its accuracy by appeal to actual specimens we have found it reliable. A lengthy classified bibliography of recent literature is appended, containing a list of papers which deal both with the specific and cognate subjects. The author is careful to explain in his introduction that this is incomplete, and that the whole hand-and-text-book literature is omitted; but, that notwithstanding, we should have expected at least a reference to the investigations of Zaaijer on the architecture of the human skeleton, to the recent remarkable papers by Cope on "False Elbow Joints" and "The Mechanical Origin of the Hard Parts of the Mammalia" (upon which we could have wished the author's expression of an opinion), and to the allied observations of Scott and others, intimately associated therewith. The results of the author's laborious researches will ere long find their way into the text-books; and now that the study of arthrology is being put upon a sound basis, it is absolutely imperative that the time-worn misrepresentations of the position of the limbs of the Eared Seals, the Walrus, and the Bats, so long reproduced again and again in popular and elementary treatises, should give place to more accurate representations of fact, if only as a necessary preamble to the treatment of the more exact knowledge of which we are now becoming possessed.

Papers and Notes on the Genesis and Matrix of the Diamond. By the late Henry Carvill Lewis, M.A., F.G.S. Edited from his unpublished MSS. by Professor T. G. Bonney, D.Sc., LL.D., F.R.S. (pp. 69, with 2 plates and 35 woodcuts). Longmans, Green & Co., 1897.

By the loving care of his widow these valuable papers of the highly gifted Professor Carvill Lewis—whose early death was so great a loss to science—have been rendered accessible to geologists. The circumstance that the proofs have passed through the hands of Professor Rosenbusch of Heidelberg and of Professor T. G. Bonney of University College, and that the latter has taken upon himself the responsibilities of editorship, afford a sufficient guarantee that the accuracy and scientific character of Professor Carvill Lewis's very interesting memoirs have been maintained.

The book includes a memoir entitled "On a Diamond-bearing Peridotite and on the History of the Diamond," read before the British Association at Birmingham in 1886, and one on "The Matrix of the Diamond," delivered before the same association at Manchester in 1877. In these memoirs the author, besides giving much valuable and original information concerning the mode of occurrence of diamonds in South Africa, establishes the existence and characters of a peculiar type of rock to which he assigned the name of "Kimberlite".

As a supplement to these papers on the South African rock, Professor Bonney has included in this little volume a chapter on the "Kimberlites" from Syracuse, N.Y. and Elliot County, Ky., in the United States. His account of these rocks is based on unpublished notes by Professor Carvill Lewis himself, and on later memoirs by Professor G. H. Williams and Mr. Diller.

It is well known that Professor Carvill Lewis contemplated a complete discussion of the relation of diamond-bearing deposits in different parts of the world to olivine and serpentine rocks. But the notes left on this subject were, in the judgment of Professors Bonney and Rosenbusch, too incomplete for publication with advantage at the present day.

Electro-Chemistry. By Max Le Blanc. Translated by W. R. Whitney. London: Macmillan & Co., 1896.

To those unacquainted with the chapters in Ostwald's *Lehrbuch* dealing with the same subject this book will prove extremely interesting, and even to those who are familiar with the views which Ostwald has developed so plausibly the book will be very useful.

Le Blanc's own researches, begun under Ostwald's direction, occupy an important position in connection with the subject, and this fact lends additional interest to the work.

The first part of the book—containing chapters on the dissociation theory, the migration of the ions and the conductivity of solutions—deals very clearly with questions already familiar to the English reader. The manner in which migration data are practically obtained is more fully described than is usual.

The second part—containing chapters on electromotive force and polarisation—will, however, attract much more attention.

It is unnecessary to refer to the contents of these chapters in detail; but one or two points are perhaps worth noticing.

The conception of solution pressure is very useful when an attempt is made to realise the nature of the equilibrium between a solution and an electrode; but the relation

$$\pi = k\,T \log P/p$$

—where P is a constant depending on the nature of the electrode—can be derived without the aid of this conception. For this reason the method adopted by Le Blanc of first introducing the notion of solution pressure and then deducing the relation above quoted is open to criticism. The sentence which ends at the top of page 154 needs qualification.

While the author makes no attempt to discuss the nature of the equilibrium between an electrode and a solution in the general case, the view which he takes of the equilibrium in some particular cases seems open to objection. For instance, in the case of an electrode of zinc amalgam considered on p. 159, the zinc may determine the magnitude of the potential difference; but no reason is given why the "electrolytic solution pressure" of the mercury should not determine

the concentration of the Hg ions in the solution. The generality of such an objection is not affected by the fact that, in the case considered the concentration of the mercury ions would be excessively small. The remarks on the *Influence of Negative Ions* (*e.g.*, p. 227) seem open to a similar objection.

In the account of the capillary electrometer, the part played by the acid is not made sufficiently clear. "Chemical Cells" are treated as in Ostwald's *Lehrbuch*, and values for the "Heats of Ionisation" of different elements are tabulated. The source of the energy transformed in the cell (apart from any heat which may be absorbed from the environment) is thus made to depend upon the variation of energy when an element changes into the ionic state. No suggestion as to the nature of the variation is made; but a paper by J. J. Thomson on "The Relation between the Atom and the Charge of Electricity Carried by It" is of interest in this connection.

The chapter on polarisation is the outcome of Le Blanc's own researches. These are of great interest and form a most important contribution to our knowledge of electrolysis generally.

In connection with the theory of gas cells, an experimental determination of the temperature coefficient at different pressures (*cf.*, p. 254) would be very useful. The peculiar position occupied, on Le Blanc's view, (1) by hydrogen amongst kation-producing substances and (2) by oxygen amongst anion-producing substances is worthy of notice. The question of the capacity of the "double layer" at the electrode surface is not considered by Le Blanc. In this respect his work is incomplete for the question is one which is intimately connected with the theory of electrolysis. A considerable amount of experimental information concerning this has been published and it would seem that the views developed in Le Blanc's book can be successfully extended to the elucidation of some of the results.

Finally, since many of the discrepancies which become evident when the phenomena are examined in detail are still unaccounted for, the physical basis of the book cannot yet be said to be perfectly secure. It may however be reasonably held that the "most important term" in the explanation of the phenomena has been discovered. The fruitfulness and utility of the "working hypothesis" can scarcely be questioned. The views developed connect and arrange many of the facts of electrolysis in a very remarkable manner and this is of the utmost importance, for "of all electrical phenomena electrolysis appears the most likely to furnish us with a real insight into the true nature of the electric current".

It should be added that the translation has been very well done, the number of misprints and minor inaccuracies being unusually small.